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MINUTES OF PROCEEDINGS

OF THE

INSTITUTION

OF

CIVIL ENGINEERS;

WITH

ABSTRACTS OF THE DISCUSSIONS.

Vol. XXVI.

SESSION 1866-67.

EDITED BY

JAMES FORREST, Assoc. Inst. C.E., Secretary.

Index, Page 585.

7755

LONDON:
Published by the Institution,
25, GREAT GEORGE STREET, WESTMINSTER, S.W.
1867.

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LONDON: PRINTED BY WILLIAM CLOWES AND SONS, STAMFORD STREET AND CHARING CROSS.

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THE

INSTITUTION

OF

CIVIL ENGINEERS.

SESSION 1866-67.

November 13, 1866.

JOHN FOWLER, President,
in the Chair.

No. 1,154.—"Results of the Employment of Steam Power in Towing Vessels on the Gloucester and Berkeley Canal."[1] By William Brown Clegram, M. Inst. C.E.

The Council of this Institution having invited a communication on the employment of steam power on canals, the Author proposes to give the results of the last four years' working of this power on the Gloucester and Berkeley Canal.

A brief description of the canal may be necessary to a right appreciation of the subject. It is a ship canal, commencing from the River Severn, at the city of Gloucester, and terminating at the River Severn, at a place called Sharpness Point, in the parish of Berkeley. It is 16½ miles in length, and level from end to end. Its top-water width varies from 80 feet to 100 feet, with many parts extending to 150 feet and 200 feet wide, to facilitate the passing of large vessels. It has a depth of water varying, according to the season, from 18 feet to 18 feet 6 inches. The width at the bottom varies from 13 feet to 20 feet. A cross section of the canal (Fig. 1) represents its general size and shape.

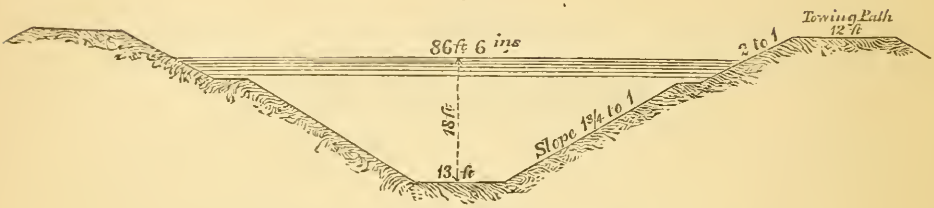
Vessels up to 600 tons and 700 tons register navigate the canal to Gloucester, drawing from 15 feet to 16 feet of water. Vessels of this size have a portion of their cargoes taken out at Sharpness

---

[1] The discussion upon this and the following Paper was taken together, and occupied portions of three evenings, but an abstract of the whole is given consecutively.

Point, which is sent in lighters to Gloucester. Prior to the year 1860, all sea-going vessels were towed by horses, the number of horses being regulated by the following scale :—

Fig. 1.



Section of Gloucester and Berkeley Canal.

For vessels not exceeding . . .	40	tons register .	1 horse.
Do. of 40 tons, and not exceeding	80	,, . .	2 horses.
,, 80 ,,	130	,, . .	3 ,,
,, 130 ,,	180	,, . .	4 ,,
,, 180 ,,	250	,, . .	5 ,,
,, 250 ,,	300	,, . .	6 ,,
,, 300 ,,	350	,, . .	7 ,,
,, 350 ,,	420	,, . .	8 ,,
,, 420 and upwards		,, . .	9 ,,

The cost of this amounted generally to about one farthing per ton per mile on the register tonnage of the vessel, but it was slightly less for the larger class of vessels. The speed varied from 1 mile to 3 miles an hour, according to the size of the vessel and the state of the weather.

In 1860, three steam tugs were placed upon the canal to do this work. They are iron boats, two being 65 feet long each, 12 feet beam, and drawing 6 feet 3 inches of water, fitted with high pressure engines, the diameters of the cylinders being 20 inches, with a length of stroke of 18 inches, the pressure of the steam being 32 lbs. on the inch. The screws have three blades, 5 feet diameter and 6½ feet pitch. The third tug is 55 feet long, 9½ feet beam, and 5 feet draft of water; fitted also with a high-pressure engine, the diameter of the cylinder being 16 inches, with a length of stroke of 18 inches, and the pressure of the steam 25 lbs.; the diameter of the screws 4 feet, also with three blades, and pitch 5½ feet. The cost of these three tugs, including fitting up and delivery, was £3,000. Two men and one boy are employed on each tug. The consumption of coals is from 15 cwt. to 20 cwt. every twelve working hours.

With the exception of a very occasional use of horses, to meet the emergency of a large and sudden influx of trade, the whole of the sea-going craft are now towed by these tugs. In the four years ending the 25th March, 1865, during which period this mode of towing has been adopted, 1,059,137 tons register of shipping have been towed 16 miles, carrying 1,109,334 tons of goods, at a cost of £6,400, including 15 per cent. per annum on the cost of the tugs, to cover interest of money, repairs, and renewals. Applying this cost to the register tonnage of the vessels towed, it gives 1·450 penny per ton for 16 miles, or ·0906, about $\frac{1}{11}$th, of a penny per ton per mile, being a saving of not far short of two-thirds on the cost of the haulage by horses. In consequence of a larger and more regular trade in the six months ending the 25th September, 1865, the cost during that period did not exceed $\frac{1}{13}$th of a penny per ton per mile. Applied to the goods conveyed in the vessels in the four years, the result is ·0865 of a penny per ton per mile.

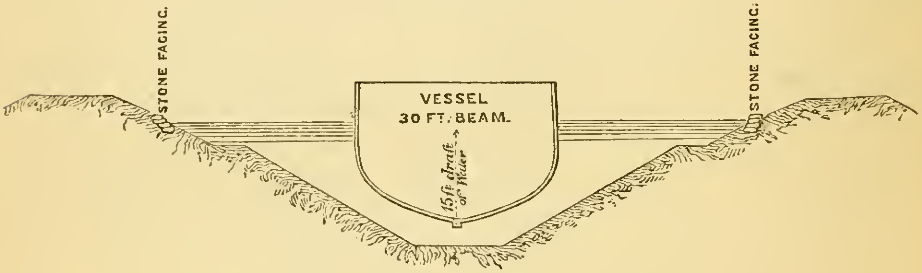
The vessels towed ranged from 30 tons up to 600 tons and 700 tons register, with a varying draft of water of from 6 feet to 16 feet. They are towed either singly, or in a train, according to circumstances. Sometimes as many as nine, ten, and even thirteen loaded vessels, of from 50 tons to 100 tons register each, have been towed by one tug at the rate of 3 miles to 3½ miles an hour. The heaviest load after any one tug has been 1,690 tons of goods, in three vessels, the aggregate register tonnage of which amounted to 1,200 tons. Their draft of water varied from 14 feet 6 inches to 15 feet 6 inches, and they were towed the whole length of the canal, at the rate of 2 miles an hour. The smaller vessels are towed at a speed of 4 miles an hour, to which, as a rule, they are restricted. The rate of speed in a canal with such a limited section is of course very variable, depending greatly on the size of the vessel, and the state of the weather.

The employment of steam as a towing-power has been found in nearly every way advantageous. The work is greatly economised. The vessels rub much less against the sides of the banks, the towing-power being right ahead, and not on one side, as with horses. The wear to the ropes used in tracking is much reduced. The speed is increased; and vessels can now be moved along the canal in weather which would have prevented horses doing the work. With a strong wind athwart the canal, vessels cannot be tracked in train; they must then be taken singly, or, at most, two only. When vessels are towed in train, as a rule, the largest and heaviest drafted are placed first, and the hawser, or towing-rope, leading from the first vessel to the tug, is taken from each side of the bow. With this arrangement, and a skilful management of the tug, the vessel can be kept fairly in the line of the canal.

Most of the vessels employ pilots accustomed to the navigation; and the larger vessels have two men on shore, the one forward, and the other aft, with checking lines, to aid their passage through the bridges and along the canal. There are sixteen bridges, having a clear width between their abutment walls of 36 feet. As one of the many proofs of the advantage and safety of steam-towing, it may be mentioned, that the damage done to these bridges, by the vessels getting foul of them in their passage through, has been reduced one-half since this power has been employed.

The one and only disadvantage of this system, on a canal the sides of which at the water's edge are unprotected, is the additional wear at this part, caused by the constant passage of the tugs, as well as by the run of water between the sides of the large vessels and the banks. Such vessels, as will be seen by reference to Fig. 2, occupy a large part of the sectional area of the canal, and being taken along at a much greater speed than they were by horses, the back run of water is more rapid and prejudicial. When the vessels, or trains of vessels, are heavy, and the tugs are working up to their full power and speed, the water thrown back by the action of the screw against the bow of the first vessel, is thrown off by it to the banks on each side, and is the cause of considerable wash. This has been attempted to be remedied by placing the first vessel farther back from the tug; but in practice it is found, that a distance of from 40 feet to 50 feet is the farthest separation that can be allowed, without sacrificing that hold between the two which prevents the vessel sheering from side to side. The first vessel being kept steadily in her course, the others follow without much difficulty.

Fig. 2.



The injury to the banks arising from the wash is confined to a depth of about 18 inches only, one-half of which is below, and the other half above the water-line. If left alone, the slipping of the bank from above would soon become extensive. To secure the banks against this injury, they are being protected by a facing of good weather stone, obtained from the Forest of Dean. It is a sandstone, and comes out of the quarries in beds of from 6 inches to 12 inches in thickness. It is packed closely together, in a recess cut in the face of the bank, about 12 inches in thickness, for a depth of 2 feet; one-half of which is below, and the other half above the water-line. It presents a true, neat face, along which the water runs harmlessly. Some portion has now been put in three years. It stands well, and has completely answered the purpose; and the only repair that it needs, is the replacing of the stones that may be accidentally removed. The cost of the stone delivered on the canal is five shillings and threepence per ton, which with the interstices left in the wall, as the stone is 'used in the rough,' represents from 18 cubic feet to 20 cubic feet; and including the labour, it can be put in for about £180 per mile. As a set-off to this expense, there is, of course, the diminished wear of the towing-path by the horses, which was considerable.

An extent of traffic has been carried on the canal during the last year, that could scarcely have been accomplished by horse-power; and so manifest is the economy and efficiency of the system, that it far more than compensates for the increased cost of protecting and maintaining the sides of the canal.

An unexpected facility has arisen in cleansing the canal from the deposit of mud, from the employment of the tugs, which may be mentioned. Formerly, it was difficult to remove this deposit from the slopes of the banks. It was dangerous to apply the dredger; and although the mud in the bottom of the canal could be removed, it would hang upon the slopes, and at times inconveniently contract the capacity of the canal. Since the vessels have been moved at greater speed, and in trains, this deposit has been entirely removed from the slopes to the bottom of the canal, whence it can readily be taken out by the dredger.

The communication is illustrated by one diagram, from which Figs. 1 and 2 have been compiled, and appended to it is a printed copy of the regulations governing the towing on the canal.

No. 1,165.—"On the Employment of Steam Power upon the Grand Canal, Ireland."[1] By Samuel Healy.

This system of canal navigation extends in a westerly direction from Dublin to the River Shannon, and in a southerly direction to its junction with the River Barrow, and is 160 miles in length. The locks are 60 feet long, and 13 feet 6 inches wide; a depth of 5 feet 2 inches of water is maintained upon the cills, but the trading depth of the boats is limited to 4 feet 3 inches. The width of the canal varies from 60 feet to 80 feet, shallowing at each side, so as to admit of about 30 feet of navigable depth in the centre. Upwards of 300,000 tons of goods are carried annually over this system, in and out of Dublin.

Steam power is applicable to canal navigation in either of two ways; and both have been attempted on the Grand Canal. 1st, By placing the machinery in the boats with the cargo; and 2ndly, By employing steam power merely for towing purposes, and hauling boats or barges in trains. Trials have also been made with both a single and a double screw, and although there are advantages attainable by the latter, principally in the facility it affords for reversing in a straight line, the disadvantages are such as to lead to the conclusion that it is unsuitable for canal purposes. There is a greater liability of accident to the propellers, in consequence of their projecting at either side,—the single screw being, from its position, more central,—and there is an unnecessary multiplication of machinery, without corresponding additional power.

In the first effort to introduce steam power, the boats were designed to carry cargo as well as the machinery; and with this object in view, in the year 1851, Mr. John Scott Russell (V.P. Inst. C.E.) was employed to build a boat, and to fit her with the necessary machinery. This boat, with a single screw, was the nearest approximation to success, with a boat propelled by steam and carrying its own cargo; but her carrying capacity was found to be so reduced, by the weight of and space occupied by the machinery, as to render the speculation unremunerative; and it was subsequently employed on river navigation in deep water. At the same period, another builder constructed two other boats, each propelled by a double screw, with the like result. The employment of steam haulage is obviously more expensive than horse power, unless boats can be towed in trains, and the requisite speed be obtained.

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[1] The discussion upon this and the preceding Paper was taken together, and occupied portions of three evenings, but an abstract of the whole is given consecutively.

In the year 1860, experiments were made in propelling by chain haulage, on a plan proposed by Mr. Robertson; but this was a total failure.

Within the last two years, there has been introduced on a long level of the canal (25½ miles), a system of hauling boats in trains, by small towing steamers. These vessels are of iron, 60 feet long and 7 feet beam, and have been built expressly for the purpose. The machinery is powerful, and is capable of hauling five boats with 40 tons of cargo in each. This is the most successful effort that has been made upon the Grand Canal. The propeller attached to the two first of these towing steamers (Fig. 3, No. 1) had a width of blade at the circumference of 22 inches, but being limited to a diameter of 3 feet 6 inches, in consequence of the depth of the canal, it was found that it was important to increase the width of the blade at the circumference to 38 inches, and to accomplish this, to increase the length of the boss. This amended form (Fig. 3, No. 3) is found to be effective, but accompanied by some loss of power, as will appear from the form of the screw. To remedy this, a screw was constructed which has been proved to be the best form for canal purposes (Fig. 3, No. 2), with a width of blade at the circumference of 32 inches, cut away at the base to the extent that is consistent with strength; the pitch is an increasing one, varying from 5 feet 3 inches to 7 feet 9 inches.

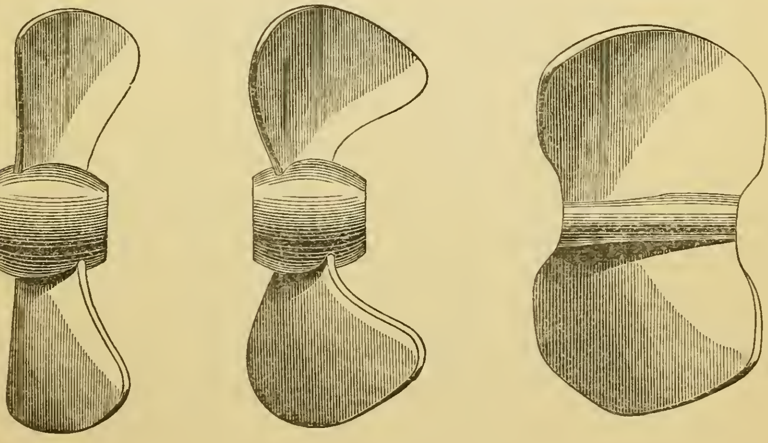

Fig. 3.

No. 1. No. 2. No. 3.

The towing steamers haul three laden boats, each carrying 40 tons of cargo, at the rate of 2½ miles an hour; the pressure on the boiler being 60 lbs., and the number of revolutions 75 to 80. A greater or a lesser number of boats is hauled at a proportionately lesser or greater speed. Five laden boats can be hauled, when required, with ease. The boats are 60 feet long, about 13 feet beam, and draw about 4 feet of water, when laden with 40 tons. On that portion of the canal upon which steam power has been introduced, horses have been withdrawn altogether, and two steamers perform a daily service both ways, on this 25½ miles of canal, regularly. The consumption of fuel, which is one part of coal to three parts of slack, is 112lbs. per mile.

The steamers, designed to carry their own cargo, on the River Shannon, have to pass through locks, which limit their length to 72 feet, and beam to 13 feet 3 inches. They carry 50 tons, with a draft of water of 4 feet 8 inches, and have been most successful.

This communication is accompanied by a Map of the Grand Canal, by a photographic view of the machinery in goods-steamers for carrying their own cargo, and by models of these steamers, as well as of the three forms of screw propeller referred to in the Paper, (Fig. 3,) and by a model of the River Shannon steamer.

APPENDIX.

GRAND CANAL COMPANY, IRELAND.

Specification of Machinery of Tug Steamers on Grand Canal.

The Engines of these Steamers are horizontal and high pressure, and are placed side by side, longitudinally with the vessel. The cylinders are 8 inches in diameter, with a length of stroke of 15 inches; the valves are wrought by link motions, for reversing the engines rapidly.

On the crank shaft is keyed a bevel wheel, giving motion to a bevel pinion on the shaft of the propeller, at the rate of 200 revolutions per minute. Neither of these wheels is a mortise one at the present time; the wood teeth having been found not to answer.

The Boiler is cylindrical and horizontal, with a single furnace and return tubes. The diameter of the boiler is 4 feet 6 inches, and its length is 13 feet 6 inches. The furnace is 2 feet 3 inches in diameter, and the fire-bars are 4 feet 6 inches long. There are forty-eight tubes, each $2\frac{1}{2}$ inches in diameter outside, and 11 feet 2 inches long. The chimney is 11 inches in diameter and 6 feet 6 inches high, and the exhaust steam is turned into it, to create a strong draught.

Specification of a pair of Engines for the River Shannon Steamers carrying their own Cargo—50 tons on 4 feet 8 inches of Water.

The Engines are direct-acting, high-pressure, of an inverted diagonal arrangement, placed at an angle of 90°. The cylinders are $7\frac{1}{2}$ inches in diameter, with a length of stroke of 12 inches. The slide-bars are four in number. The connecting-rods are of wrought-iron. The reversing link motion is worked by one handle on deck. It is of wrought-iron, deeply case-hardened. The eccentrics are solid. The force-pumps are of cast-iron, of the same quality as the cylinders. The shaft is of wrought-iron, of the best description, and 4 inches in diameter; the crank is single.

The Screw-propellers are of the common Smith's form, of wrought-iron, 4 feet in diameter; each screw is two-bladed, and one is placed before the other; each blade being $\frac{1}{9}$th of a convolution. The leading screw has a pitch of 7 feet, and the after one a pitch of 8 feet, arranged at right angles to each other on the shaft, and 9 inches apart.

All the pipes connecting the regulator and the cylinders, the cylinders and the blast-pipe, the pumps and the boiler, are of copper, and the flanges of brass.

The Boiler is made of Low Moor iron, or iron of equal quality, the plates generally being $\frac{1}{2}$ an inch thick, and flanged and rivetted together; the tube-plate is $\frac{11}{16}$ths of an inch thick, the front plate $\frac{5}{8}$ths of an inch thick, and the smoke-box tube-plate $\frac{5}{8}$ths of an inch thick. The shell of the fire-box is $\frac{1}{2}$ an inch thick and 3 feet 6 inches in diameter, and is made of iron of an approved brand. The fire-box is 2 feet 9 inches in diameter and 3 feet 8 inches long. The tubes are of iron, 2 inches in diameter outside, and 5 feet 6 inches long between the tube plates. They are seventy-seven in number, and are secured by light steel ferrules at both ends. The smoke-box doors are in parts, and open from the centre to the side, with defending plates inside.

[Mr. James Milne.

Mr. James Milne, of the Forth and Clyde Navigation, remarked, through the Secretary, that ten years ago, and for some years after, he had occasion to give this subject his attention, in endeavouring to introduce steam power on the Forth and Clyde and Monkland Canals; and, at the third annual meeting of the Canal Association, held in Birmingham, on the 4th August, 1858, he exhibited drawings of steam engines for a canal goods lighter, for a canal and sea-going lighter, for an ice-breaker, and a single engine for a goods boat or mineral 'scow.' These boats and their machinery were thus described in the Proceedings of that meeting:—

"The canal goods lighter is one of a class which carry 80 tons; the engines are two 6¼ inch cylinders, with 10 inches stroke of piston : the boiler an upright tubular boiler, 3 feet diameter; weight of engines and boiler, with water, 2½ tons. The engines and boiler are capable of working to 100 lbs. pressure on the square inch; but 35 lbs. pressure in the cylinders is found sufficient for propelling the lighter, with cargo, at the rate of from 4½ to 5 miles an hour. The engines were completed in September, 1856, since which time they have been regularly at work; they were made of sufficient power to propel the lighter, and to tow one or two similar lighters; but the traffic the lighter has been employed in has not afforded opportunities for using the surplus power in towing. The engines and boiler, with stern-post and propeller, cost £320; a considerable proportion of the cost and also the weight being due to the surplus power.

"The ice-breaker, in addition to breaking the ice on the canal, is also used as a crane boat and service boat for the canal works. The steam engines for this boat were completed in May last, and consist of two cylinders 9½ inches diameter, with 15 inches stroke of piston, with two upright tubular boilers 3 feet diameter, all calculated for being worked to 100 lbs. pressure. Cost of engines, boiler, and propeller, £430.

"The engine for the goods boat or mineral scow is not yet put to work, but is contracted for. The engine, a single 6-inch cylinder, with 9 inches stroke of piston; the boiler (an upright tubular boiler) 2 feet 6 inches diameter, all to be capable of working to 100 lbs. pressure. It will not cost more than £150.

"All the vessels referred to were formerly towed by horses: they are the usual canal vessels, and have been and are being pierced and fitted for screw propellers. The canal and sea-going lighter is now being prepared for receiving the engines and propeller.

"Since the goods lighter was put to work in 1856, there have been five lighters fitted with steam power put to work on the canal by the canal traders."

Again, at the next meeting of this Association, on the 16th August, 1859, he furnished a report relative to these canal steamers, in which he observed that:—

"The following are the principal dimensions of engines, &c., of the canal craft above referred to (see next page):—

"There are now eighteen vessels propelled by steam working on the canal, and not fewer than half that number are being fitted with steam power for the canal.

"The length of the Forth and Clyde Canal, from Grangemouth on the Forth, to Bowling on the Clyde, is 35 miles, with a branch 4 miles in length to the Monkland Canal at Glasgow, joining the main canal at near the 26th mile from Grangemouth, and the Monkland Canal is 12 miles in length from Glasgow to Woodhall. The greater amount of the traffic on the Monkland Canal is

Name or Designation of Vessels.	Cargo carried in tons.	Number of Cylinders.	Diameter of Cylinders.	Stroke of Piston.	Maximum Pressure required in Cylinder for full cargo.	Diameter of Screw Propeller.	Pitch of Screw.	Boilers Upright, No. of.	Diameter of Boilers.	Height of Boilers from F. B. to Crown.	Length of Tubes.	Taper Tubes inside Diameter.	Heating Surface in square feet.	Speed with average Load. Miles per Hour.	Space occupied by Engines and Boilers from Stern Post to Bulkhead.	Time when put to work.
			ins.	ins.	lbs.	ft. in.	ft. in.		ft. in.	ft. in.	ft. in.	ins. ins.			ft. in.	
'Thomas' .	80	2	6¼	10	35 to 40	3 6	4 0	1	3 0	7 6	3 6	2¼ & 1¾	110	4½ to 5	11 0	Sept., 1856
'Marjory' .	35	1	6	9	45 to 50	2 8	3 3	1	2 6	5 8	2 6	2⅛ & 1¾	53	5 to 6	9 0	Feb., 1859
Ice Breaker	say 80	2	9½	15	. .	4 9	5 3	2	3 0	7 9	3 6	2¼ & 1¾	220	3 to 6	13 0	May, 1858
120 ton Lighter	120	2	9½	14	To be worked to 60 lbs. at sea.	4 9	5 3	2	4 0	9 0	F. Boxes with Pouches	. .	200	5 in Canal 10 at Sea	13 0	Now being made.
Scow 'James'	60 to 70	1	7	9	60 to 75	3 0	3 4	1	2 6	5 8	2 6	2⅛ & 1¾	53	2½ to 4½	8 6	Aug., 1859

from the Coatbridge and Gartsherrie mineral district, a distance of about 10 miles from Glasgow, and the usual carrying of minerals is by the scows, which are worked on the Forth and Clyde Canal by two boatmen, one horseman, and one horse; and on the Monkland (when carrying to Glasgow only) by one boatman, one horseman, and one horse; and the usual carrying on the Monkland Canal to Glasgow by the scows is, down loaded with from 55 tons to 60 tons, and up empty, which is done at a cost of about 9½*d.* per scow per mile, the scow loaded half-a-mile and empty half-a-mile.

"With steam power say for the 20 miles:—

	£	*s.*	*d.*
One boatman, @ 4*s.*	0	4	0
One engineman, @ 3*s.*	0	3	0
Coals, 4 cwt., @ 4½*d.*	0	1	6
Oil and tallow, 3*d.*, gasket and waste, 1*d.*	0	0	4
Cost of engine, £150, 5 per cent. on per day	0	0	6
Ditto for depreciation and repair	0	1	0
Cost per trip of 20 miles	£0	10	4

Gives about 6¼*d.* per mile. Some of the steam scows carry a lad for giving greater facility in passing the locks; wages of lad, say 2*s.* per day, which added, gives 12*s.* 4*d.*, or nearly 7½*d.* per mile; but this mileage, while it is more than an average day's work for a horse, is not a full day's work for the scow, with engine power, and this short journey may be taken as most unfavourable for the comparison of steam propulsion with horse haulage. The steam power gives a more just and favourable comparison on longer journeys, say from Coatbridge to Grangemouth, which gives 10 miles on the Monkland Canal, and 30 miles on the Forth and Clyde Canal, together 40 miles from Coatbridge to Grangemouth. This is a usual trip in the working of the mineral traffic, and the trip loaded 40 miles, and back empty 40 miles, in all 80 miles, is usually esteemed a week's work for a scow and crew towed by a horse, but the trip is sometimes done in five days by horse haulage, and with steam power the trip is accomplished in three days.

"Cost per trip by horse haulage, say—

	£	*s.*	*d.*
Five days of man and horse, with towing ropes, @ 7*s.* 6*d.*	1	17	6
Five days of 2 boatmen, each @ 4*s.*	2	0	0
	£3	17	6

"Cost per trip by steam power, say—

	£	*s.*	*d.*
Three days of 2 boatmen, each @ 4*s.*	1	4	0
Ditto engine driver, @ 3*s.*	0	9	0
Ditto per centage on £150, @ 5 per cent.	0	1	6
Ditto, ditto, for repairs, &c., 10 per cent.	0	3	0
Coals, 16 cwt., at 4½*d.*—6*s.*; oil, 1*s.*; gasket and waste, 4*d.*	0	7	4
	£2	4	10

"It will be observed that on trips of such length as the above, there is a great saving of time, and it is estimated that with trips of average distance two boats propelled by steam will do as much work as three boats towed by horses.

"The Forth and Clyde Canal, communicating with the Friths of Forth and Clyde, offers facilities for employing the carrying craft propelled by steam to advantage in carrying beyond the canal, and boats of the scow class furnished with hatches have sailed to Leith and to Greenock, distances of 24 miles and 12 miles from the termini of the canal."

The fact of steam power having been tried at various times on the Forth and Clyde and the Monkland Canals, from Symington's steamers in 1789,—the 'Charlotte Dundas' in 1801, and the more recent trials by other parties—did not favour the attempt made in 1855; and in his proposal to employ carrying steamers, with light high-pressure engines, he found that he had so far to conform to the instruction of the Canal Board, as to make the trial steamer of sufficient power to tow two lighters similar to the lighter in which the engines were fitted. To towing he would have had no objection but for the numerous locks on the navigation, and the delay which must take place at the locks, both to a tug with its fleet, and to promiscuous canal trade. On the Forth and Clyde Canal, which was 35 miles in length between the Frith of Forth and the Frith of Clyde, there were forty locks with a rise of about 8 feet each, and at distances apart of from 50 yards to 17 miles, lock to lock. On the Monkland Canal there were ten locks, eight of which were double locks, each having a lift of 12 feet, giving a rise of 96 feet, with basins between the pairs, the other two locks having a rise of 10½ feet each, with a basin between. The locks being so placed, and thus numerous, and the canals being free to all traders on payment of tolls—with a considerable and increasing trade—and the Canal Company not being carriers, except in the single instance of a limited trade in goods, he considered that an attempt to collect and tow the various descriptions of craft belonging to the traders offered no chance of success, while he feared that towing the vessels in fleets might delay the general trade, and give just cause of complaint. He therefore, with the concurrence of the Canal Board, resolved on making a trial of carrying steamers with small high-pressure engines and screw-propellers, the engines being fitted as close to the stern of each vessel as practicable. The lighter 'Thomas,' which was fitted with engines and put to work in 1856, and carried 80 tons of cargo, was used for the goods-trade worked by the Canal Company. The engines and the boiler had been constantly at work since that time, and were still working most satisfactorily, but little repair having been needed either for the boiler or the engines, and no renewals either for the boiler, the fire-box, or the tubes.[1]

The application of steam power to the 'Thomas' having proved successful, engines were designed and fitted to the luggage-boat 'Marjory,' carrying 35 tons; to one of the canal ice-breakers; to masted lighters for canal and coasting trade, carrying 120 tons; and designs for a 'scow,' or mineral barge, carrying 60 tons on the Monkland Canal, and 75 tons on the Forth and Clyde Canal, all of which had proved successful, and had been the precursors of about seventy canal steamers now at work on the Canal, and from the Canal to the contiguous sea coasts.

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[1] Some particulars of this lighter, by Mr. Neil Robson, M. Inst. C.E., are to be found in the Transactions of the Institution of Engineers in Scotland, vol. i., p. 49.

The increase in the number of steamers on the Forth and Clyde Navigation Canal and the Monkland Canal was shown in the following table:—

1856	1 steamer.	1862	36 steamers.
1857	2 ,,	1863	44 ,,
1858	7 ,,	1864	50 ,,
1859	18 ,,	1865	58 ,,
1860	25 ,,	1866	70 ,,
1861	30 ,,		

Mr. Edwin Thomas said, through the Secretary, that the Regent's Canal Company, in the year 1854, issued an advertisement offering a premium of £100 for the best tug-boat which should be put in competition on a day to be appointed by them, and £50 for the next best boat. In August, 1855, a trial took place, which resulted in the first prize being awarded to Mr. Inshaw, of Birmingham, for the screw tug-boat 'Birmingham,' which was now the property of that Company, and which was, until June, 1865, constantly employed in hauling the barge traffic upon the summit level of their canal. The vessel was 6·68 feet wide, 70·68 feet long, and drew 3·50 feet of water. It was fitted with a multitubular boiler and an engine having a pair of cylinders 7 inches diameter, and was worked with steam at a pressure of 60 lbs. to 70 lbs. per square inch. Two screws of 4 feet pitch were fixed near to the stern end of the vessel, and were revolved in opposite directions to each other, by means of bevel wheels, in the proportion of two to one. (Plate 2, Figs. 2 and 3.) The capabilities of this vessel would appear from the following statement of the work performed on the 15th June, 1862:—Started at 6·15 a.m., and at 7·45 p.m. (13·5 hours) had run over 11·25 miles of canal, towing twenty barges, seventeen of which were laden with an aggregate amount of cargo of 931 tons. If to this be added the weight of the twenty vessels, which might be taken at an average of 15 tons each, the gross load would be 1,231 tons—exclusive of the weight of the tug. At one trip it hauled eight vessels, containing 421·5 tons of cargo, over a distance of 2·25 miles in 3 hours 45 minutes. The sectional area of the water way traversed, as compared with that of the vessels navigating the canal, was about 4 to 1; except for the portion of canal through the Maida Hill Tunnel, which was only about 2 to 1 for a distance of 270 yards. The cost of working that vessel for the eight months ending 31st May, 1865, was £344. 2*s.* The distance traversed was 3,519 miles; number of barges hauled 2,023; the gross amount of cargo conveyed was 59,738 tons, which, with the weight of the barges, each averaging 15 tons, made a total of 90,083 tons towed. The result of the work performed during the period stated was—

Cost per train mile, labour and repairs only	1·26	shilling.
„ „ „ fuel only	·70	„
„ „ „ including all charges	1·96	„
Cost per ton of cargo, labour and repairs only . . .	·893	of a penny.
„ „ „ fuel only.	·49	„
„ „ „ including all charges	1·383	„
Cost per ton, gross weight, labour and repairs only . .	·593	of a penny.
„ „ „ fuel only	·323	„
„ „ „ including all charges . .	·916	„

The consumption of fuel was rather large, owing chiefly to the want of a larger boiler.

From the consideration which Mr. Edwin Thomas had given to the question of applying steam power on the Regent's Canal, he was inclined to believe that it could not be economically employed by the canal traders, and therefore would not come into general use, unless some plan were adopted for combining the tug and cargo vessel, so that they might pass through the locks together, and be readily separated at the termination of the journey. His views on this subject were reported to the Canal Association in 1859 in the following words:—

> "One suggestion I would offer on this subject, *viz.*, that the vessels to be propelled by this method of propulsion should be constructed in two parts; the total length not to exceed that of the present vessels in use, which are capable of passing through the locks in canals; the part containing the machinery should, I propose, be as short as it may be practicable to have it, and form the hinder or stern portion, and be fitted with the rudder; the stem or fore end of the vessel to be constructed of a circular, or any other shape that may be considered best. I also propose that the after or stern of the other portion be made exactly the reverse of the fore end of the first-mentioned vessel, so that the two parts may be joined together at those ends, and so form one continuous line, . . . the ends of the vessels being provided with couplings, for securing the vessels together and detaching them."

The advantages to be derived from this plan were, that the propelling machinery would not be restricted to one vessel, but could be applied to any vessel of a similar shape. By this arrangement it could be constantly kept at work, thereby effecting a considerable saving in the expense of haulage; besides obviating the necessity of a large expenditure in applying similar machinery to all vessels proposed to be propelled by the means referred to, or that of the ordinary screw. In this way he thought the chief impediment to the more general application of steam power to haulage purposes on canals would be removed.

Mr. Clegram said, although the Paper had been written about eight months, he did not know that he had much to add to it, except to state that one of the main difficulties, where two or three large vessels were towed together, was to keep the last vessel in line with those before it. That was remedied by adopting the practice of sending three or four rafts of timber behind the last vessel, which were found to have sufficient hold upon the way of the last vessel to keep it tolerably in line with those that preceded it, and in that manner the traffic had been conducted for the last four years. The total amount of work performed by four tugs, another one having been placed on the navigation recently, during three weeks in September, 1866, gave more advantageous results than those stated in the Paper. In those three weeks 35,280 tons of goods were moved 16 miles on the Canal by the four tugs, at the following cost:—

	£.	s.	d.
81 tons of coals at 14*s.*	56	14	0
Tallow, oil, cotton waste, &c.	9	0	0
Wages £3 2*s.* per week for each boat	37	4	0
Agents employed at each end of the Canal £130 a-year, say	8	0	0
Interest of money, and wear and tear, 15 per cent. per annum on £4,000, for three weeks . . .	34	10	0
Total . . .	£145	8	0

These figures represented the actual outlay for working, except in the case of the amount charged for tallow, oil, cotton waste, &c., which was estimated. That cost of £145 8*s.*, applied to the number of tons conveyed, came to not quite one-sixteenth of a penny per ton per mile, an economy beyond what was stated in his Paper, where it was said to be one-twelfth of a penny per ton per mile, showing that where steamers were regularly employed it became a very economical mode of moving vessels on the Canal.

Mr. E. Leader Williams said, his experience with reference to steam towing had been chiefly upon the river Severn, which was very different to tugging in still water, as there was a velocity of current of sometimes 5 miles an hour, and with a stream usually running down, when there were any freshets, at from 2½ miles to 4 miles an hour. Steam tugs had been used on the Severn for the last ten years. In the first instance the Commissioners had purchased one of a pair of tugs that had been employed in towing coal barges on the Thames, from London to Windsor, named the 'Enterprise,' the other being called the 'Perseverance.' The engines were from 30 HP. to 40 HP., with reefing paddle wheels.

The 'Enterprise,' when used on the Severn, answered well. She had brought from Gloucester trains consisting of twelve vessels, carrying cargoes of 30 tons each, at a rate of 2½ miles to 3 miles an hour, against a stream running 2 miles an hour; and she had done that work very economically. He had not experienced on the Severn the difficulty alluded to in the first Paper, of keeping the train of vessels in line; this was no doubt owing to the action of the current against which the vessels were towed. For the first 2 miles out of Gloucester, the Severn was narrow and tortuous; but still he had no difficulty in bringing trains of twelve vessels behind the tug, reaching of course a considerable distance down the river; and while the tug was going in one direction, the tail of the train would sometimes be advancing in an opposite one. Nor had he experienced the difficulties mentioned with regard to passing locks. At the last lock he built on the Severn, he constructed, in connection with it, a large basin, with a pair of gates; so that by the use of the basin and the lock together, he had passed the steam tug and a train of nine vessels at the same time, and very expeditiously. A number of tugs of different construction had recently been introduced upon the Severn; but the most efficient he considered to be those which carried a cargo and towed as well. These were barges of 70 feet in length, 12-feet beam, and 3½ feet draught of water; fitted with a pair of small, direct-acting engines, with cylinders 7½ inches in diameter, and a length of stroke of 9 inches, working direct upon a pair of twin screws, 2 feet 6 inches in diameter. These vessels were capable of carrying 40 tons of cargo, and of tugging two or three canal boats with 30 tons of cargo in each. One of those steamers, with one canal boat behind her, had recently passed him, at the rate of 2½ miles per hour, against a stream running from 3½ to 4 miles per hour. He thought that was the most efficient mode of towing upon the Severn; and he recommended the adoption, in all cases, of engines on board the cargo vessel, instead of towing with detached tug vessels, as from his experience it was more economical in working than any other plan. For instance, the vessel he had alluded to burnt from 1½ to 2 cwt. of Staffordshire coal per hour, when carrying 40 tons of cargo on board, and towing two canal boats carrying 30 tons each: that was moving 100 tons of cargo at an expenditure of 2 cwt. of coal per hour, which was the maximum consumption of fuel.

Mr. Pole wished to mention a case that had come before him some years ago, and which was interesting in some points regarding the use of steam power on inland canals. Some coal-owners trading on the Ashby-de-la-Zouch Canal, thinking that it offered a good opportunity for the use of steam haulage (there being a length of 30 miles without any lock), proposed to convey their coals in this way; and ordered a steamboat from Mr. Inshaw, of Birmingham, who, shortly before, had patented a mode of propulsion by a double screw at the stern, and had received a reward for his invention from the Regent's Canal Company. The Midland Railway Company, however, who were the proprietors of the Canal, refused to allow the boat to ply, on the ground that the steamer would damage the works of the canal, and interfere with the navigation; and this refusal led to the institution of proceedings in Chancery to test the rights of the case. As information on the subject was wanting, the Master of the Rolls directed that an Engineer should be instructed to make experiments upon the canal with the proposed boat, in order to see what effects might be expected to arise from its use, and this question was referred, by the consent of both parties, to Mr. Pole.

He accordingly tried the experiments in the month of May, 1859, with a boat called the 'Pioneer.' This was of the ordinary size used on the Canal, namely, about 70 feet long, 7 feet wide, and 4 feet deep. It had been fitted by Mr. Inshaw with a small steam engine of 6 HP., working twin screws at the stern; the machinery took up but little room, and left a large portion of the boat available for cargo: the boat was also intended to be used, if necessary, for towing. The experiments comprised many different varieties of conditions, in regard to the loading of the steamboat herself, and the number of other boats she had in tow; and the speed attained varied from 1¼ to 5 miles per hour; this speed varying, not only with the work done, but also with the section of the Canal, as it was found that where the Canal was diminished in sectional area the speed was retarded, and *vice versâ*.

The principal object of investigation was to observe the wave, or surge, caused by the passage of the steamer, and to estimate the effect it would have on the banks of the Canal. It was well known that any vessel propelled through a confined channel would produce, in passing, a wave or oscillation of the surface of the water, which would probably, in some shape or other, reach the banks, and cause an agitation of the water in contact with them. The magnitude and character of this agitation would, however, depend on the velocity and the other circumstances of the case. It might be so trifling as to be practically harmless, or it might take such a form as to be capable, if frequently repeated, of producing much injury. These two conditions merged so gradually into each other, that it was difficult to define the exact limit where the harmless state ended and the injurious one began; but, after careful observation, Mr. Pole was led to define the commencement of the injurious action to take place when the wave began to assume a breaking form, as distinguished from a convex shape, or wave of translation; and he conceived that when a continuous wave possessing this character in a marked degree accompanied the boat, injury to the banks, if not protected by masonry, or otherwise, must in time be expected to occur.

This point being fixed, observations were directed to the character of the wave produced at different speeds; but it then became necessary to draw a distinction between two causes, by either or both of which waves might be produced. These were, first, the actual passage of the boat through the water, which would be independent of the means of propulsion; and secondly, the action on the water of the propelling apparatus itself, which (as might be seen by watching the passage of a paddle-wheel steamer in the Thames) was capable, under certain circumstances, of creating a wave more formidable than that due simply to the motion of the body. Now it was a very decided result of these experiments, that the method of propulsion, by the double screw, did not, of itself, at any speed attained, give rise to any wave or surge at all injurious to the banks of the Canal. The two screws caused, of course, an agitation of the water in their wake, but this did not extend to the banks in any such form as could damage them. This second cause of wave was therefore dismissed from consideration. There then remained the wave arising from the passage of the boat through the water. This varied in some degree with the section of the Canal, and with the occurrence of curves in its direction; but it principally depended on the speed, and the following were the general results obtained. Up to a speed of 3 miles an hour no wave of an injurious character appeared. Between 3 miles and 3½ miles an hour a breaking wave appeared occasionally in curves and shallows. Above 3½ miles an hour the breaking wave became more continuous, and took a more marked character. At 4 miles an hour the injurious character of the wave became very decided. At 5 miles an hour, even in a much enlarged section, the wave was still more increased, breaking sometimes over the towing-path, and being followed by other waves in succession.

Mr. Pole did not find, in the course of these experiments, that the use of the steamer, with ordinary care, caused any injurious interference with the general navigation by other boats. It had the advantage of the absence of a tow-line, and of being able to pass on either side other boats at pleasure. The only inconvenience he could imagine was in case a tug was made to draw a long train of boats at a very slow speed, by which the passage of other traffic through narrow bridges, or tunnels, or locks, might be delayed. The result of the investigation was to lead Mr. Pole to recommend that the steamboats should be admitted upon the Canal, subject to such a limitation of their speed as would avoid the production of an injurious wave; and this recommendation was made an order of the Court of Chancery.

Mr. John F. Ure said, about about fourteen years since, when the modern improvements of widening and straightening the channel of the Clyde had made but little progress, that river, particularly in its upper part, was narrow, at low tide shallow, and in several places very tortuous. It was, however, used by vessels of almost all sizes and kinds then constructed, but by the largest at high water and in daylight only. The sailing vessels and large steamers always proceeded at a slow speed, but the river passenger steamers at the quickest rate they could travel. These passenger steamers were about 160 feet to 180 feet long, 16 feet to 18 feet beam, 5 feet to 6 feet draught, propelled by engines of 80 HP. to 100 HP.; and some, as the 'Iona,' a well-known specimen of these river steamers, exceeded these dimensions and power. The channel of the river in the upper part, near Glasgow, at low tide, was about 120 feet to 150 feet wide at the level of the water, with sloping sides protected by stone, and about 10 feet deep. These steamers in the open sea attained a speed of from 16 miles to 18 miles per hour; but in the narrowest and shallowest parts of the low-water channel the rate did not exceed 8 miles to 9 miles per hour, and at this speed a very great swell and surge in the water were produced. It was quite usual, at such times, to observe the water commencing to rise when one of these vessels was 2 miles or 3 miles off. This it did gradually, increasing as the steamer approached, and more rapidly near the vessel, till the wave broke, which it generally did opposite the steamer, but always upon the shoal water adjoining the river walls, against which it accumulated and was alone high; in the centre of the river it was inconsiderable. Upon the shore at the base of the walls, the hollow of the wave was considerably under low water. That part of the wave in the shoal water broke with great violence against the river walls, and necessitated the use of heavy whinstone rubble facing, from 2 feet to 3 feet thick, proceeding from below to above high water, and hand-built on the face, to protect the banks, and prevent them from being washed away. This great surge being the governing strain on the works, attention did not require to be directed to the lesser strains produced by the other vessels moving at slower speeds.

The scouring action produced by the surge from these steamers extended to the bed of the river. As an instance, it might be mentioned, that although the steamers were compelled to slacken speed when within a distance of a quarter or half a mile from where the diving-bell was in operation, yet it was necessary to raise the bell several feet. This scour was one of the principal means of enlarging the navigable channel; for example, on a length of 7 miles below Glasgow Harbour, where the river walls were tolerably perfect, and the surge produced by the motion of these vessels was therefore confined, the soil being of various kinds, but mostly sand, the navigable channel was increased during the fourteen years, 1839 to 1853, below low-water level to the extent of 1,176,035 cubic yards, of which only 459,952 cubic yards were removed by dredging, the remaining 716,083 cubic yards of enlargement being due to the scouring action produced by these vessels moving at the highest speed possible in such a channel. It might be said that this increase was due to other causes, as the scouring action of the tide, freshes, &c.; but Mr. Ure was quite satisfied that the enlargement was not due to these causes. One means he took to ascertain this was, to have the increased capacity of channel when dredging a particular work, where the tide did not exceed ½ a mile an hour, measured at regular intervals in the river, and the quantity produced in embankment on the shore; when he found the quantity so produced in the embankment uniformly less, whether the work was performed in the dry summer or wet winter weather. Speaking from recollection, he believed from 20 to 25 per cent. was washed away in the performance of the work. The length of the river in which these results were produced might be divided into two parts: the upper part of 2½ miles next the harbour, where a more considerable quantity of dredging had been performed, partly in straightening the bends, &c., had been increased in capacity 588,880 cubic yards with a dredging of 383,200 cubic yards; and in the lower 4½ miles, where the main, or only, dredging had been to remove the shoals, as the scour produced a general enlargement, an increased capacity of 587,155 cubic yards was effected with a dredging of 76,752 cubic yards. A similar enlargement had taken place in the remainder or lower part of the channel of the river; but being wider and its walls less perfect, in some cases there being none at all, the increased capacity due to the scour was not to the same extent.

The early part of the wave, the part before it commenced to break, he considered was a wave of oscillation, but after it commenced to break, a wave of translation, moving with great force. He was satisfied that it was common, at the period referred to, for such waves in the narrow part of the river at low water, especially when the steamer was of the largest size and approached one side of the river, to measure (vertically), from the hollow in the channel to the crest on the wall, quite 8 feet or 10 feet. This wave was still considerable on the Clyde in the narrow parts at low tide; but with the increased channel, both in depth and width, it was much less than formerly.

With these views he did not think any great speed could be attained in a canal or limited river channel, by vessels of large size; and he believed that if such vessels were propelled beyond a slow rate, their progress would produce abrasion both of the banks and bed of the navigation.

Mr. G. H. Phipps said, about twelve years ago experiments were tried under his direction upon the Stoke Canal, in Staffordshire, with the system of propulsion by the ejection of water. He did not think it was tried as it ought to have been, and therefore it was no discredit to that system that it was not then successful. The conclusion arrived at was that it was not superior to the common screw. He had designed a screw which was so arranged that it could be raised and lowered, by being fixed on a shaft with a universal joint, to suit the varying draught of the boat, that being one of the difficulties of canal navigation. With a vessel of that kind good service was done, and the expense was just about the same as the cost of haulage by horse power; but there being only one of those vessels, it was insufficient as a system, and the consequence was, from various reasons, it was not carried further. It appeared to him that a necessary element to make this system advantageous was a considerable length of canal without locks. On the Grand Junction Canal, where there were many locks, great delay and loss must necessarily arise; and this would also be the case where the canal was of small sectional area. With regard to the economy of screw propulsion in a narrow canal, no doubt the screw worked to great disadvantage from the enormous slip under such circumstances. It was well known that a high amount of duty was only obtainable with a screw when it was put in rapid motion, whereby it got hold of a great deal of water to operate upon. In a vessel going only 2 miles an hour, the screw operated on the same water over and over again. On the Grand Junction Canal the speed when towing one vessel did not exceed 3 miles or 3½ miles an hour: there must therefore be a loss of power in that application. He thought it might be worthy of investigation, whether in such an instance as this—though he considered in larger cases the Ruthven system a bad one—with a narrow section and slope of bank of the ordinary kind, some system like that of Ruthven, producing momentum by a certain quantity of weight taken in per second, might not be more advantageous than the common screw.

Mr. E. E. Allen said, with regard to steam tugs on the Thames, some years ago his attention was called to the subject by Mr. Thomas Page (M. Inst. C.E.), then acting for the late Prince Consort, who was desirous of having tug boats on the Thames at Windsor, to avoid the necessity of horses passing by Windsor Castle. Mr. Allen, after examining many tug boats in England and Scotland, came to the conclusion that the best thing for the Thames was the paddle wheel, with feathering floats, immersed 3 feet below the surface. He had an experimental boat built with engines of 30 HP., working at 40 lbs. pressure, and with that vessel as many as ten barges had been frequently towed at a time from London to

Richmond and Teddington, where three or four were left, and six were carried on as far as Oxford. The next boat built was of the same character, with engines of 40 HP., and that constantly towed three, four, and six barges against the stream, which ran about 3 miles per hour. Those steamers were worked profitably for four years, until the whole of the traffic disappeared from the Thames, and went to the Great Western Railway. The cost was $\frac{1}{10}$th of a penny per ton per mile. He thought the use of the screw was not possible in that case, on account of the depth of the water. The slip was rather great, more especially where the river was narrow. Taken as a whole, those boats succeeded admirably, and nothing but the discontinuance of the traffic on the river would have caused them to stop running. When it was found that the traffic was likely to cease, the boats were sold to the Commissioners of the River Severn; one was lost on the passage, but the other was still at work. For shallow rivers, where there was a considerable breadth of stream without much depth, paddle wheels were perhaps the best; but where there was sufficient depth, the screw would create less disturbance of the water. Immense opposition was experienced in introducing those boats. Probably one of the greatest obstacles to the development of steam power on canals was the smallness of the locks, which generally admitted of only two barges being locked at one time; but in the instance he had referred to this was overcome.

Mr. Leader Williams, jun., said, he was now engaged in the construction of works on the River Weaver—interfering with those of Telford and Cubitt—with a view of bringing steam power to bear. In consequence of railway competition in the conveyance of about a million tons of salt per annum, independent of the coals required for its manufacture, he recommended the trustees to remodel the river, so as to render it navigable by large vessels, and to introduce steam tugging to its full extent. The Gloucester and Berkeley Canal was a favourable instance of what might be done with steam, and it was satisfactory to hear that, along a length of 16 miles without locks, vessels could be conveyed at such low rates as $\frac{1}{12}$th to $\frac{1}{16}$th of a penny per ton per mile, especially when it was considered that there was no iron permanent way to be kept in order. On the Weaver a length of 24 miles was divided into eight pounds, some as much as 6 miles, and others only half that distance, apart. A few years ago the traders introduced steam tugs, but without success. He had just finished duplicating the locks, which were now 100 feet long by 23 feet wide, and capable of admitting vessels of 8 feet draught and carrying 150 tons. It was found that even with double locks, the delay with steam tugs towing several barges was so serious, and so interfered with the traffic, that that plan had to be given up. About two years ago, steam barges conveying their own cargoes were tried, and were so successful that others had been introduced. These were 85 feet long, 19 feet 6 inches beam, drawing about 7 feet 6 inches water, and carrying from 180 tons to 200 tons in each. It was found, with direct-acting engines of 20 HP., that there was no difficulty in towing two or three lighters carrying 100 tons each, behind the barge; but then, of course, there was delay at the locks. It was clear if that difficulty could be removed the traffic would pay well: and the trustees had obtained an Act for raising £200,000 for the improvement of the navigation. A third lock was about to be made for each pound. This lock was to be 200 feet long and 40 feet wide, and it would enable each tug to take three barges through each lock. In this way not only would the disadvantages of ordinary locks be overcome, but a positive gain would be effected by taking four vessels through instead of only one. It would be a canalised river, partly river navigation and partly a long canal. The top water minimum width, which at present was about 60 feet, was to be increased to 90 feet, while at the bottom the width would be 54 feet, with a depth of 12 feet. That section, compared with the Gloucester and Berkeley Canal, would give great advantages. The character of the soil was better, and he believed the work would be carried out with slopes of 1½ to 1. It was hoped by these measures, not only to have steam barges, but coasting vessels up to Northwich. He could not give the cost per ton per mile, as the traffic was in the hands of the traders. On the Weaver sailing vessels and hauling by horses were being abandoned, and the steam barges he had alluded to were considered to be most profitable.

Mr. Hubert Thomas said, through the Secretary, that the engine boat 'Dart,' the property of the Grand Junction Canal Company, was built of timber, and was 70 feet in length, 7 feet beam, and drew, when loaded, about 4 feet. The length occupied by the engine cabins and decks was 31 feet, leaving for the stowage of goods 39 lineal feet. The distance travelled between 1st October, 1864, and 1st October, 1865, was 11,280 miles; the number of tons carried and hauled was 3,182; the working expense, including the engine and accompanying boat, was £366 13*s*., which was equal to 7·8 pence per train mile, and ·184 of a penny per ton per mile of cargo conveyed.

The vessel was fitted with an 8 HP. steam engine, and a Griffiths' three-bladed screw propeller of 3 feet diameter. The engine was constructed at the Company's works, under the superintendence of, and according to designs prepared by, Mr. Elliott. The cylinder of the engine was 9 inches in diameter, with a length of stroke of 8 inches, and was worked expansively, with the steam cut off at half-stroke. The pressure in the boiler was 75 lbs., and the speed 180 revolutions per minute. The cylinder was vertical, and acted directly upon the shaft of the propeller. The connection of the engine with the shaft was so arranged, that in the event of any uneven wearing of the bearings no undue strain was brought on the engine, which considerably lessened the risk of accident from that cause. To pump the bilge water from the engine room, and for feeding the boiler with water, a small compact donkey engine was fixed to the side of the boat, in the engine cabin, and had a double action, one for feeding the boiler, the other for pumping the water from the vessel in case of need. This engine could be worked with a pressure of steam in the boiler of 5 lbs. To it also was attached a water-heater, in which the temperature of the water was raised to about 200° before it passed into the boiler, thus effecting a great saving in the consumption of fuel, and producing an even working pressure. The vertical flue boiler attached to the engine was of the following dimensions:—Height 7 feet, diameter 4 feet 3 inches, diameter of fire-box 2 feet 10 inches, and 2 feet 10 inches from the fire-bars to the crown of the fire-box; the depth of the flue being 3 feet 4 inches, and the width 3 inches, and the water spaces 2 inches. The internal flue, or chimney, was 4 feet in length by 9 inches in diameter. The area of the fire-bar surface was 5 feet, with an effective heating surface of 120 feet. The shell of the boiler was constructed of the best Staffordshire plates $\frac{3}{8}$ of an inch in thickness, and the whole of the interior of the boiler was constructed of Low Moor plates also $\frac{3}{8}$ of an inch in thickness. The boiler, before being placed in the boat, was tested with a pressure of 150 lbs. to the square inch. The exhaust steam was rendered serviceable, in preventing the emission of smoke from the chimney, by an arrangement of pipes, fitted with a valve, the pipes passing into the funnel of the boiler. The result was that no smoke was allowed to escape, which was very desirable when passing through long tunnels.

Mr. W. H. Bartholomew said, through the Secretary, that steam haulage was introduced on the Aire and Calder Navigation so far back as the year 1836. It was conducted by means of paddle tugs, having high-pressure engines; the cylinders, two in number, were 11 inches in diameter, and had a length of stroke of 20 inches. The paddle wheels were 9½ feet in diameter by 3 feet 6 inches wide. Internal flue boilers were used, working at a pressure of from 50 lbs. to 60 lbs. per square inch. The cost per boat per mile hauled was 8·516 pence, and per ton per mile ·473 of a penny. The speed with three boats, containing 100 tons of cargo, was 3 miles per hour in the canals, and 4 miles in the rivers.

The employment of steam power on the Aire and Calder was confined to the main line between Leeds, Wakefield, and Goole—a distance of 36 miles. Between Leeds and Goole there were ten locks, having a total fall of 66 feet, and between Wakefield and that port there were seven locks, with a total fall of 50 feet. Between Goole and Castleford the locks occurred at intervals of 9 miles, and above that place at intervals of about 2 miles. The depth of the canals was 8 feet 6 inches, and of the rivers 9 feet to 10 feet. The top width of the canals was 60 feet to 66 feet, that of the rivers 100 feet to 150 feet. The bottom width of the canals was 30 feet, the sides having slopes of 2 to 1; the average sectional area was 380 square feet. Improved means of steam haulage were introduced in the year 1853, which had been continued and extended to the present time. The method of propulsion was the screw. Two systems of employing it were adopted, viz., that of the tug carrying cargo, in addition to its tugging capabilities, and that of the tug having increased power, acting solely as a tug. The first-mentioned class of tug was confined to the merchandise traffic. The dimensions of these tugs were—length, 63 feet 6 inches; beam, 12 feet 6 inches; depth, 7 feet 6 inches; capacity for cargo, 30 tons. The machinery occupied 20 feet in length of the after part of the vessel. The engines were high pressure, direct acting, on the inverted diagonal arrangement. The cylinders were 8½ inches in diameter, and had a length of stroke of 12 inches. The boilers were tubular, fitted with copper fireboxes and brass tubes. They had 12 square feet of grate surface, 26 square feet of fire-box, and 217 square feet of tube surface; the working pressure was 100 lbs. per square inch. The propeller was 5 feet 3 inches in diameter, and 7 feet pitch, making about 180 revolutions per minute. The average cost of haulage by steam tugs for the past seven years had been 2·125 pence per boat per mile, and generally ·085 of a penny per ton per mile. This traffic was conducted during the night, at an average speed of 4·5 miles per hour, at which speed the canal banks sustained no injury.

The second class of tug was solely employed for towing the general traffic. The dimensions of the vessel were similar to those described for the merchandise traffic. The machinery occupied the whole of the vessel, except so much as was set apart for the crew. Its arrangement was different to that previously described. The engines were direct acting. The cylinders were inverted, and placed overhead. Their diameter varied from 15 inches to 18 inches, and the length of the stroke from 12 inches to 16 inches, and the working pressure was from 60 lbs. to 80 lbs. These tugs were fitted with two propellers, on the same shaft, 6 feet in diameter, set some distance apart, and at right angles to each other. The leading propeller had a pitch of 7 feet 6 inches, and the after one of 8 feet 6 inches. The boilers were return tubular, with two fire-boxes, having the tubes beneath the latter, and had, for the cylinders of 16 inches in diameter, 16½ square feet of grate surface, 116 square feet of fire-box, and 804 square feet of tube surface. They would tow ten keels, having 700 tons of cargo, at 3 miles per hour in the canal, and 4 miles per hour in the river. The charge for towing was at the rate of $\frac{1}{10}$ of a penny per ton per mile against the stream, and $\frac{1}{12}$ of a penny per ton per mile down stream.

Two vessels fitted with steam power, and capable of carrying 160 tons each, had been recently set to work; but the short time they had been in operation did not justify more than a passing notice. A new mode of carrying minerals had been lately introduced by Mr. Bartholomew. It consisted of a train, composed of seven rectangular boats, having their ends constructed with an outward curvature of 6 inches. The dimensions of the boats were, length, 20 feet, beam varying from 15 feet to 16 feet, and depth, 7 feet 3 inches. Each compartment, or boat, was capable of containing from 25 tons to 35 tons. When formed into a train, they retained their lateral position by means of a projecting stem, which fitted into a corresponding hollow stern-post. They were held together and steered by wire ropes, which passed through suitable guides on each side, and which extended from the steam compartment at the after end to the leading or stem portion at the other. They were tightened by hydraulic power, and when together formed a train, or vessel, 190 feet in length. They were steered by two steam cylinders, having their pistons in direct connection with the wire ropes, and were found to answer well in all respects. Each compartment was fitted with spring buffers at its corners. The compartments were discharged by hydraulic power, which raised the compartment and its cargo, weighing about 42 tons, to the elevation required to suit the height of the ship. At this stage of the operation the compartment was gradually turned on its side, and the contents discharged into a large shoot, and thence into the ship. In this way 100 tons to 120 tons per hour had been shipped; but this quantity was entirely ruled by the sizes of the ships' hatchways and the number of trimmers.

Mr. R. P. Brereton said, he could not gather from Mr. Healy's Paper why the particular form of screw adopted was considered the best. It was stated that preference was given to a width of blade of 32 inches, with a sharp taper; but whether that preference was on account of towing purposes, or as regarded the disturbance of the bottom of the Canal, did not appear. In the first Paper the damage to the canal banks was attributed to the action of the paddle wheels; and it was remarked that this action was confined to a width of about 18 inches along the banks, partly above and partly below the water line. He was anxious to ascertain whether, after a lengthened employment of the screw propeller on these navigations, there had been any opportunity of examining the condition of the bottom of the Canal, and also what the bottom consisted of.

He was led to ask for that information owing to a circumstance which happened to the 'Great Eastern,' from working her screw in shoal water. She was placed upon the gridiron at Liverpool, and her screw was worked to back her off it and move her astern. After several revolutions of the screw, the ship started, but she had not proceeded above 80 yards, on the incline of the gridiron, before she pulled up and grounded so as to lose the tide. On inquiring into the cause, it was found that the blades of the screw had cut a hole in the sandstone rock about a yard in depth, which ultimately stopped her. That being the case, he could scarcely imagine but that considerable disturbance to the soft bottoms of canals was occasioned by the action of the screw, at whatever speed it might be worked; and upon that point, as well as the nature of the bottom, he should be glad to receive some information.

Mr. Michael Scott said, in estimating the cost of propulsion, regard must be had to the relation between the size of the vessel and the dimensions of the water-way. In the case of a vessel passing through the water at a given velocity, the resistance would be less in a large water-way than in a small one. In other words, a vessel which would create considerable disturbance in a narrow canal, would pass through a large channel without creating any appreciable wave. Another point which occurred to him was, that the eroding power of the wave would be greater upon an extended slope of bank than upon a vertical face.

Mr. J. F. Bateman remarked, that on the Aire and Calder Canal the screw propeller had been worked since 1853. A few years ago he made an examination of the banks of that Canal, and up to that time no mischief had been done. The average depth of water was upwards of 6 feet. The precise draught of the vessels, in proportion to the depth of water in the Canal, he did not remember; but he knew there was considerable space between the bottoms of the vessels and the bottom of the Canal. He might mention that, on the Forth and Clyde Canal, some years ago, before the introduction of steam tugs, a very convenient class of canal boat, capable of carrying 80 tons of cargo, and of being drawn by one horse at a rate of 3 miles an hour, was employed. The cost of haulage was then about $\frac{1}{12}$th of a penny per ton per mile; but in that case the Canal was 9 feet 6 inches in depth, and the draught of the boats was 5 feet 6 inches to 6 feet, leaving upwards of 3 feet between the bottom of the boat and the bottom of the Canal. Being a ship canal, of great width, the water had free means of escaping by the sides, as well as below, so that no surge was created, as was the case in narrow canals. That was a matter to be regarded in the question of haulage upon canals.

Mr. Robert Mallet remarked, that he was concerned, a quarter of a century ago, in some rather large experiments on the Irish canals, of which no account had as yet appeared. As the results obtained were, in some respects, remarkable, he would state that, about the year 1836, Mr. Hunter introduced on the Scottish canals, fly boats capable of carrying sixty passengers, when towed by two horses. About two years after, Mr. Mallet was instrumental in getting that description of boat adopted upon both the Grand and the Royal Irish Canals. They carried sixty passengers at the rate of about 8 miles an hour, were towed by two horses, and occasionally by three. The distress to the horses was, however, considerable, owing to the wave of translation, which travelled along with the boat when at full speed, passing on a-head when the boat pulled up, and so requiring to be re-established when the full speed was to be restored. The late Mr. Charles Wye Williams, and Mr. Watson (Assoc. Inst. C.E.), the present managing director of the City of Dublin Steam Packet Company—but then the manager of the inland department of that Company, comprising the Canal and Shannon Navigation, for goods on both, and for passengers also on the latter—were thus interested in improving the passenger boats on the canals in the hands of the Canal Companies, and of ascertaining how far this could be effected by the substitution of steam power for that of horses. Mr. Watson had patented a canal boat capable of parting in two in the middle, and jointed in a peculiar manner, so that the two halves could be placed parallel to each other, and so be passed through a lock of 70 feet long, although the boat when together was about 120 feet in length. The object was to get a vessel of the smallest beam and the smallest transverse section of displacement, so as to offer the least resistance in the narrow waters of the canals. The proposal was then laid before Mr. Mallet, to adapt steam power and paddle wheels to this boat, in place of horse power. The aim, of course, was to pack into a vessel of that kind, so narrow in beam and crank, the greatest power that she was capable of containing, and to apply it so as to obtain the highest speed possible. The conditions of the problem were, that if a speed of about 8 miles an hour could be obtained, with a cargo of sixty passengers and their luggage, so that the journey from Dublin to Shannon Harbour could be made in one day, it would be a success; and it was believed it would then answer commercially. The boat was of such extremely small beam —5 feet 9 inches—so crank and so flimsy in build, that there was great difficulty in putting adequate power into her, or in obtaining sufficient foothold for machinery in a boat of such length, built of half-inch oak planking. He designed for this boat high-pressure engines capable of being wrought up to about 40 HP., with a boiler on the locomotive plan. The weights were distributed, by the aid of longitudinal trussed keelsons, over as large a floor as possible, to enable the boat to sustain them. The entire weight of engines and boiler in working trim did not exceed 5 tons—screw propulsion, it would be remembered, was then unknown. The diameter of the paddle wheels was much limited by the height of the bridges to be passed under, and the width of the floats by the narrowness of the locks. The form of paddle floats he adopted was that proposed by the late Mr. George Rennie, which were then believed to possess some peculiar properties in going easily into the water and lifting very little of it. Each float was of the shape of the section of an egg, dipping point down. Various different shaped and sized paddle floats were afterwards applied to the same engines, with slightly different radii of paddle arms, and different dips, and with varying, and in some cases apparently anomalous, results. With a load equivalent to sixty passengers and their baggage, a maximum speed of 7·08 miles an hour was attained, with the original oval float boards, 24 inches deep and 17 inches wide, and with the engines working considerably below their full speed. Several curious and anomalous facts were observed. The boat was tried upon both canals—one with a section of 40 feet and the other of 44 feet water surface—flat bottom of 25 feet, sides sloping about 1½ to 1, and mid-depth 6 feet to 6½ feet of water. When the boat was put in motion the speed was rapidly brought up on either canal, without notable disturbance, to 6 miles an hour. A wave was then produced, the crest of which crossed the canal close in front of the boat, which never rode upon it or over it. It was not a wave of translation, for the speed of such a wave, due to the depth, 6 feet to 6½ feet, of these canals, was about 8 miles an hour, which the boat never reached. Although 7 miles an hour was the speed with about half the power of the engines, when they were worked up to full power, the result was a tremendous surge at the sides and rear of the boat, but no distinct increase of speed. In fact, a better result was obtained with 32 strokes per minute than with the maximum of 55; the only effect of the additional number of strokes being to create violent disturbance of the water in the boat's wake. At the bridges, where the Canal suddenly narrowed to the width of the locks, or to 14 feet, and where the greatest amount of resistance might have been expected, the engines (for the second or two while the paddles were passing the spot) flew away, showing that the back current of the water, required to fill the comparative void in the wake of the boat, took away the fulcrum from the paddle wheels, and was one of the causes of defective speed. There was a peculiarity in the form of the after or following wave that was created by the action of the paddles and passage of the boat. It went off at an angle of 30°, from the tail of each paddle wheel towards the bank, and was there reflected sometimes as many as five or six times after the boat, producing a set of waves crossing in a lattice form; but there was no disturbance ahead beyond a simple roll or pushed-up wave of about 9 inches, as already referred to. One curious experiment was made by attaching three picked and powerful fast post horses to the steam-boat, capable of keeping a strong strain upon the tow line while the engines were at work. On one occasion he started the engines and four horses at the same time, when the speed of the boat was rapidly brought up to 10 miles an hour, and that rate was maintained for perhaps 300 yards or 400 yards, the engines flying away and the horses being scarcely able to make speed enough to keep the tow lines taut. The true wave of translation was now soon produced, upon which the boat for a short time rode. This increased in magnitude, and very soon brought down the speed of the boat to its own rate, or to 8 miles an hour. Throwing off the horses when the speed was highest, it was almost immediately reduced to about 5 miles an hour, and until the water got tranquil could not be restored. Mr. Charles Wye Williams, who, as was well known, was the patentee, in an early stage of steam navigation, of a peculiar form of paddle wheel with feathering floats, the floats being of the oar shape, passing deeply into the water edgeways, and coming out also edgeways, was anxious that this form of paddle should be tried, and a pair of paddle wheels of that form was fitted to the same engine and boat. The result, as Mr. Mallet anticipated, showed no improvement in speed; but in place of the after surge taking the form it did with the fixed float boards, as previously described, the surge was now right across the canal, not far off the stern, and it followed the boat in a continuously breaking wave. In conclusion, he would remark, that it was quite certain from what was now known—thanks to Mr. Scott Russell and others—of the nature and laws of production and motion of waves of translation, that the height of the wave above the fluid service of repose measured about the depth of the water disturbed by the wave below the normal water level. The height of this wave in these canals never exceeded 9 inches at the speed of 7 miles per hour; but with a speed of 8 miles an hour no doubt there would be produced as much or possibly more surge than that of the fly boats—viz., from 15 inches to 18 inches. It followed then, that the depth of the water disturbed by the wave, or the depth to which its disturbance could have any sensible effect in injuring the banks, could not be more than from about 2 feet to 3 feet. From this it resulted, that if the shallow water at the edges of canals, of the section above described, was cut off by vertical walls, of whatever construction might prove cheapest, so as to have a depth of about 4 feet of water at the wall, at either side of the canal, no surge of the dimensions and character above described could do any injury to the bottom or channel of the canal. To meet the scour due to the rush backwards of water to fill the wake of the boat, the total transverse section of the canal must be relatively large to the immersed cross section of the boat. The angle of the slopes of the Irish canals was originally that due to from 2 to 1 to 1½ to 1, but by the action of the water and of time, the sloping sides had become irregularly curved into a rolling batter.

Mr. Abernethy observed, that the canals which had been alluded to were not originally designed for steam traction, but merely for horse haulage. The consequence had been, in some cases, even where steam power had not been employed, but the sectional area of the boats had been enlarged, an increased height of wave and greater destructive action upon the banks. In other cases, where the section of the boats had not been increased, but steam power had been applied to propel them at a greater velocity than by horse power, the same results had taken place. There was not at present, so far as he was aware, a single instance of a canal specially adapted for the application of steam power; and it was desirable that a Paper should be prepared on that subject, describing the best form of canal for the purpose, having regard to the due proportions between the sectional area of the canal and that of the boats, together with the form of bank which would best resist the action of the waves, and the arrangement of locks to enable a train of boats to pass in and out with facility. He was struck with the result of the action of the wave in this case, which seemed to show that the slope should have a rolling or curved batter. He was some years ago engaged on the Aire and Calder Navigation for the late Mr. George Leather. As it had been stated that the banks of that canal only suffered slightly from the waves caused by steam haulage, it might be interesting to notice that the section on which he was employed, from Wakefield to Lake Lock, had a width at the top of 74 feet, and at the bottom of 31 feet, with a depth of water of 7 feet 3 inches. There were dwarf retaining walls at the sides, having a depth of 1½ foot of water against them, and then a slope of 2 to 1, with a rolling or curved batter.

Mr. Mallet wished to supplement one fact he had omitted. The same boat to which he had alluded was, at the suggestion of Mr. Wye Williams, brought upon the comparatively open waters of the Liffey, at the port of Dublin, where she attained an estimated speed of about 10 miles an hour, whereas that on the canals never exceeded 7·08 miles.

Mr. Vignoles remarked, that the principle enunciated by Mr. Mallet, as deduced from theory, as to a nearly vertical retaining wall of a certain depth at each side, was no doubt that of the method best calculated to resist the action of the water upon the banks of a canal.

Mr. Mallet said, what he meant to convey was, that the disturbance produced by a wave of translation raised by a solid body passing through a canal would not extend sensibly deeper in the water of the canal than the height of the wave over the canal when at rest. If, therefore, the banks were protected down to that depth, nothing below it would be seriously affected by the scour or disturbance produced by such a wave.

Mr. Vignoles apprehended that was an inference drawn from theory, not from practice, but it was no doubt correct. As the result of Mr. Mallet's observations, he recommended that form of section. Mr. Abernethy had shown that a retaining wall had been successfully applied, under certain circumstances, to a depth of 18 inches. Following out that principle, if the depth were increased to 3 feet, with that form of canal, although the cost would be somewhat greater, yet the beneficial results, on the larger canals, where steam was likely to be employed, would, he believed, justify the adoption of such a step.

Mr. F. J. Bramwell remarked, that in the first Paper it was stated that one of the engines employed had a cylinder of 20 inches diameter, and a length of stroke of 18 inches, with 32 lbs. pressure of steam: and that another engine had a cylinder 16 inches in diameter, and a length of stroke of 18 inches, with only 25 lbs. pressure. He gathered that in both instances these engines were non-condensing. He wished to enter his protest against the employment of such implements as non-condensing engines working at 32 lbs. and 25 lbs. of steam. Many people undertook to make steam engines, and many allowed them to be used, who did not consider that when non-condensing engines were employed, the steam had to be raised to the pressure of the atmosphere before any work could be done, and then it had to be raised further to do the work; and that the smaller this further raising, the greater was the percentage which the atmospheric pressure (representing loss) bore to the whole. With non-condensing engines working 25 lbs. and 32 lbs. steam, the pressure of the atmosphere was from three-eighths to about one-third of the pressure producing work. The cylinders of non-condensing engines should be of such dimensions, and the expansion employed should be of such an amount, that at least 60 lbs. to 80 lbs. pressure of steam should be used, or, better still, 120 lbs., and then the original atmospheric pressure became reduced to one-fourth, one-fifth, or one-eighth of the pressure doing the work. He felt compelled to apologize for speaking on a matter so well known; but he did not like to let the statement in the Paper pass without an expression of opinion as to the impropriety of the practice.

Mr. E. A. Cowper said, in reference to the haulage of boats on canals, that he had had opportunities of observing the action of water in canals on the passing of steamboats at various velocities, particularly Ericsson's experiments with different propellers, and on the Birmingham Canal Navigation with screw propellers. He believed that, for good speeds, it was inadmissible to throw off large quantities of water from the bows, to form heavy waves against the banks. He was convinced that some better plan of passing the water by the boat, would be the best means of getting the boat through the water at a fair speed, and without washing the banks unnecessarily. If a canal was only of moderate section and depth, and the section of an ordinary boat passing through it was large in proportion to that of the canal, the water displaced from the front of the boat must flow along the canal in a contrary direction to that of the boat, and thus pass along between the boat and the sides of the canal. The consequence was a considerable rush of water past the boat, and as the boat had to travel through water passing backwards, it really was going through the water at a greater speed than its rate of progress. On one occasion he had noticed that, as the steamboat was passing through a canal, the water began to rise, or 'bank up,' fully 100 feet ahead of it (as could be seen by the reflection of the light on the water); and the water being thus caused to rise by the pressure of the bows, there was formed a head of water in front of the boat, which, as it escaped from the bows, rushed past the sides and made a very considerable wash, particularly just astern of the boat, where the water was flowing into the hole left by the boat. It appeared to him, therefore, that if a hole could be dug, as it were, in front of the boat, or the water be taken away so as to keep it down to the exact level of the canal, and be put in behind the boat, there would be less obstruction to the passage of the boat than by forcing the water sideways, and obliging it to run very fast past the boat. An experiment was tried with two boats, one of which was being towed by a horse alongside the other, which was a steamboat, and it was found that the steamboat could not pass the other boat, because the propeller made a hole in the water in front, and banked it up behind, thus helping it forward. If the Ruthven, or other propeller, acting by the emission of water, were adopted, the water ought to be taken from the bows of the boat, passed through the boat by a pipe, and be ejected at the stern, thus making a hole in the water in front of the bows, or at all events preventing any resisting head of water being formed in front, and at the same time filling in the hole formed in the water by the passage of the boat, or at least preventing there being any diminution in the head of water pressing against the stern of the boat. If the water were taken through a pipe equal to the section of the boat, there would be no disturbance of the water; but as a pipe of that size could not, of course, be adopted, one of much smaller section might be used, as the water might very well pass through it at a much higher speed; indeed a high speed was necessary in any case with an emission propeller, in order to overcome the friction of the boat through the water. Probably it would be best to take the water in at the bows through perforations, so as to distribute the draught of water over the surface, which would otherwise, or under ordinary circumstances, exert a pressure against the water; or it might be found advisable to make a kind of funnel of the head of the boat for the entrance of the water to the pipe. The emission of the water at the stern might likewise be through a number of perforations, or through a single pipe, more or less tapered. It was probable that a screw propeller, or Appold's pump, would answer the purpose of drawing in water through the pipe and throwing it out again. Of course he was aware that there would thus be some sacrifice in tonnage, but the object to be obtained, viz., a good speed with steamboats on canals, or restricted channels, without injury to the banks, was of such vital importance to canal companies, that it would be well worth some rearrangement of cargo in order to attain it.

Mr. Beardmore said, some of the remarks he might have offered had been anticipated. Steam navigation on the smaller inland canals was full of difficulties. The question was, how to get the largest carrying capacity with the least sacrifice of space in the vessel, adapting thereto steam-propelling power, with fair speed? As a rule, the inland canals in this country might be taken as having a width at the top of about 45 feet, and a depth not exceeding 5 feet, though generally not more than 4 feet, with a vast number of locks on their course, frequently only admitting boats 7 feet 6 inches in width and 70 feet in length. If anything like trains with steam tugs were attempted, the loss of time in passing each vessel separately through the locks was so great as practically to prevent the successful adoption of that mode of transit. From this delay, and the difficulties offered by resistance in the restricted water-way, all attempts to work by steam trains on the canals of this country had failed. Steam trains had been tried, on an extensive scale, on the Shropshire Union Canal, with sufficient capital at command; but the experiment was a failure, from the difficulties encountered in passing the locks, and getting through the shoal-water sections of the canal. The same causes had operated against the success of steam-towing on the Kennet and Avon, the Leeds and Liverpool, and many other canals. The Grand Junction Canal Company alone still worked a portion of their traffic by steam traction. He had prepared a diagram (Plate 1, Fig. 1) to show the difficulties of getting a vessel to move through the water when there was a restricted area of channel, and why it was necessary to have ample depth and width of water-way, if steam power were to be applied successfully in a commercial point of view. The difficulties arose from a strong backward current that must pass between the vessel and the banks, and which operated in retarding the vessel, from the form of the vessel necessary for giving carrying capacity, and from the close approximation of its bottom to the bed of the canal preventing a proper supply or 'feed' of water to the screw; and this varied with the velocity, diameter, and pitch of the screw, and with the form and relative proportion of the diameter of the screw and the sections of the vessel and of the canal. To give some idea of the difference of speed where there was a sufficient water-way, and the contrary, he might state that in running with one of these small steamers in a narrow part of a canal, if an open part, or a ballast hole, were suddenly reached, the steamer would shoot a-head so as to throw down a person standing carelessly: this had occurred to himself from the steamer passing over a ballast hole.

The sections, Nos. 1 to 5, Fig. 1, Plate 1, showed the proportions of various river navigations and inland canals. The smaller class of canals had a water-way of about 108 feet of sectional area, and when the midship section of the boat was 20 feet, the proportion between the two was 5·4 to 1; some larger canals had 150 feet of sectional area, so that with the same midship section of boat, the proportion was as 7·5 to 1. On the smaller sections steam power must work at great disadvantage. Figs. 2, 3, and 4, Plate 1, showed the elevations, sections, and plans of a steamer now in daily use on the River Lee. The width of the beam was 13 feet, with from 3 feet to 4 feet draught. On some sections of the navigation its speed was not more than 2½ miles per hour; indeed he had known it to be as low as 1½ mile, while in more extended sections, the speed would be 4 miles to 4½ miles per hour, and with less consumption of fuel than when the speed was only 2 miles an hour. In steam navigation on canals, in addition to the difficulties of restricted section, there were the locks, which governed the size of the boats and their capacity for freight. The problem was how to drive an almost rectangular box through the water. In the section exhibited, besides a slight point to the bows, and a limited run to the stern, the angles where the sides joined the floors (technically called the 'chines') were rounded so as to enable the water to reach the screw more readily. This would give a practical exemplification of the difficulties encountered in applying steam for the carriage of freight. He had advised the trustees of the River Lee, to build a steam barge adapted to the general trade of that navigation, to suit as a model, and to show how economically work could be thus performed. Mr. Milne, of Glasgow, assisted him, and a vessel was built 13 feet wide, and so that the draught should not exceed 4 feet. To obtain a better run for the vessel, and to allow the water to pass easily to the screw, the barge was made with circular chines, in this respect differing from the ordinary barges on this river, which had square chines. The result was to take off at the same draught about 10 tons of carrying capacity, including the weight of the engines (about 3 tons), when compared with the usual barge freights. Thus traders had reason to complain, that only 500 quarters of malt, &c., could be carried, while with the old barges 600 quarters could be taken. The preference on the Lee navigation remained at present with the old system; and considering that an ordinary horse could tow 60 tons to 70 tons of freight a distance of 27 miles in fourteen hours, and that the crew consisted of only a skipper and one man, besides the horse-boy, there was little margin for economy. The entire expenses of taking cargo to London in this way amounted probably to one halfpenny per ton per mile.

At present, however, this navigation laboured under the disadvantage of having 8 miles of its length of the small section, No. 3, Fig. 1, Plate 1, carrying only 4 feet of water on the old lock cills. When this portion of the navigation was completed to the section No. 2, which with portions of No. 1 section formed about 19 miles of the River, it would be practicable to apply screws of 5 feet in diameter, and cargoes of 90 tons to 100 tons might be conveyed. The steam barge in question could run from Hertford, or Ware, to London with from 55 tons to 60 tons of cargo, and return with a freight —the whole distance being from 65 miles to 70 miles, including from 10 to 15 miles on the Thames—with a consumption of about one ton of coals, and at a total expense, including wages of the men, depreciation for wear and tear, and interest on capital, of one-third of a penny per ton per mile. On the larger section the speed was over 4 miles an hour; on the narrower section, including the stoppages at the locks, the speed was not more than 3 miles an hour.

In Mr. Bartholomew's communication, it was stated that the average cost of haulage by steam tugs had been 2·125 pence per boat per mile, or ·085 of a penny per ton per mile, where several barges were taken by one steamer, many of which carried 90 tons. On the River Lee one-third of a penny per ton per mile embraced every expense of a steam barge carrying her own cargo alone. While in the case of the Grand Junction Canal steamer, the expense was 7·8 pence per train per mile, and ·184 of a penny per ton per mile of cargo conveyed. The difference he attributed chiefly to the limited section of that Canal, as shown by section No. 5, Fig. 1, Plate 1, the steamer not carrying more than 20 tons of freight, and hauling about 28 tons besides. Engines with a detached boiler, and one vertical cylinder only, were used on the Grand Junction Canal; in other respects they were similar to those described by Mr. Appleby in Figs. 4 and 5, Plate 2.

From these examples, and others which have been cited, it would be seen that the cost of working depended to a great extent on the section of the Canal, as this governed the speed of the vessel. In some cases there were greater facilities for steam tugging than in others. On the Regent's Canal there was a 2-mile level at the head of the canal, on which portion alone steam had been applied; on this head-level there were two tunnels without towing-paths, and when the tug-boat was not used the vessels had to be 'legged' through those tunnels—an operation still carried on to a great extent in this country, although causing delay in transit. On the Lee he put a towing-path wall by the side of the River, wherever practicable; and this converted section No. 3, Fig. 1, Plate 1, into section No. 2, Fig. 1, Plate 1. By this a depth of from 3 feet to 4 feet of water was brought up to the wall, and from 8 feet to 10 feet of width was added to the water-way. In the altered section the same steamer with the same power would run at least $\frac{3}{4}$ of a mile per hour faster. This plan afforded great facilities for craft passing on the River; and enough space could generally be got out of the old towing-path and slopes, to obtain the additional breadth of canal, and yet leave 8 feet to 10 feet of path. The depth of the wall was from 5 feet to 6 feet; it was carried out in lengths of from 40 feet to 60 feet at a time, and with care no more water percolated to the foundations than could be commanded by a hand-pump. Some miles of the Lee had been treated in that way, with great advantage in the after maintenance, as well as from the additional room afforded. At the tidal entrance of the Lee Navigation, near the Thames, at Limehouse, he had recently built walls in the same way from 14 feet to 16 feet deep; and in carrying out the work he had no better dam than was afforded by driving temporary piles 6 feet or 8 feet apart, and camp-sheeting within them. The old facing assisted to form the dam; behind which the trench was sunk, with close runners, 11 inches by 3 inches, and the material excavated was thrown out in front. The wall was then built in the trench with Kentish rag-stone set in Portland cement concrete for the face, the backing being in blue lias lime. The section of the wall was 5 feet at the base, and 2 feet 6 inches at the top. After the walls were built, the temporary piles and planks were removed, and the excavation dredged. The whole work cost about thirty shillings per lineal foot.

The engines in the steamer to which he had alluded were from the model of Mr. Milne, and were admirably designed. The vessel had been applied to every kind of use for five years, both for freight work and as a lighter for earth and stone work, being frequently grounded every tide in the Thames, yet neither engines nor boilers had cost sixpence in repairs. He thought it might perhaps be better to allow 12 inches more length in the engine room, to give the driver elbow room, and rather more coal bunker. This reduction of the cargo space would not practically reduce the freight. The details of the engines and the form of the steam barge in question were shown in Figs. 2, 3, 4, 5, and 6, Plate 1. The length of the barge was 78 feet, of which 7 feet were occupied by the engine; the depth was 5 feet at the centre, and 5 feet 9 inches at the ends, with $2\frac{1}{2}$ inches additional of keel; breadth, 13 feet 2 inches; thickness of iron plates, $\frac{1}{4}$ inch, with ribs $2\frac{1}{2}$ inches by $2\frac{1}{2}$ inches by $\frac{5}{16}$ of an inch; internal diameter of boiler, 3 feet; height, 6 feet 4 inches; thickness of boiler plates, $\frac{3}{8}$ of an inch. There were fifty-five tubes 2 feet 6 inches in length, tapering from 2 inches in diameter at the top, to $2\frac{3}{8}$ inches at the bottom. The cylinder was $5\frac{1}{2}$ inches in diameter, with a length of stroke of $9\frac{1}{2}$ inches; the diameter of the screw was 3 feet 4 inches, and the pitch about 4 feet. About one hundred and twenty revolutions per minute gave a speed of 2 miles to $2\frac{1}{2}$ miles per hour in the cuts, 3 miles to $3\frac{1}{2}$ miles per hour in the largest sections, and 4 miles to 5 miles per hour in the Thames, with a cargo of 50 tons to 60 tons, but the speed was not sensibly increased when the vessel was in ballast. The engines might be taken generally to represent those in use on the Forth and Clyde Canals, as described in Mr. Milne's communication; but the depth of that navigation admitted a much larger screw. One of the difficulties of steam navigation on small canals, where the traffic was not very continuous, resulted from weeds; they abounded between June and September. Some of these gathered round the screw, and could only be cleared by continually reversing the screw, which, with the simple form of direct engines and link motion, was done by the helmsman as readily as by the driver. The 'silk weed' was also very troublesome, from its choking the valves of the force pumps.

Mr. Bateman remarked, in corroboration of Mr. Beardmore's calculation of the cost of working by steam, that on the Forth and Clyde Canal between Bowling and Glasgow, where there was a rise of 150 feet, and a considerable number of locks, the cost of working the system of steam lighters was 0·23 of a penny per ton per mile.

Mr. Beardmore suggested, with reference to the system of cargo trains introduced by Mr. Bartholomew on the Aire and Calder Navigation, that the plan was a complete novelty, inasmuch as the train was moved like a caterpillar, being steered by strained wire ropes, leading from the stern to the head. These ropes were also the means of connecting the rectangular boxes into one flexible train driven by a stern compartment, holding a powerful driving-engine screw, and also steam pistons for steering the head of the train by the side ropes. The whole design was well worthy of a special communication from the inventor. Mr. Beardmore had accompanied one of the early trials, and it was then a perfect success, the speed being 4½ miles an hour with a train of barges 190 feet long, and carrying 400 tons of coal; but the freights had since been considerably increased. At Goole the train was broken up, and each vessel was passed under a hoist with hydraulic gear, which lifted it above the level of the ship, and shot out the contents at one operation. This plan was more especially applicable to such freights as coals, minerals, or spoil, and the conditions of success depended on long levels, few locks, and those of great length; to the latter of which the Aire and Calder Company were now devoting considerable expenditure.

Mr. J. F. Delany described a vessel, called the 'Connector,' which was built at the Victoria Foundry, Greenwich, in the year 1858. The length was 105 feet, breadth of beam 8 feet, and depth 5 feet. She was made in three separate compartments, each compartment being 35 feet long, and forming a separate boat. These were connected together at the sides by wrought-iron pins 4½ inches in diameter, working through hinges riveted to the sides. The several compartments could be connected and disconnected in one or two minutes. She was propelled by a high pressure 10 HP. engine and a single screw, and was employed for a considerable period in the coal trade between London and the North. The speed attained in the River was 6 knots per hour. It occurred to him that a modification of this plan might be applied to canal-boats; for there was this advantage in the system, that instead of having the wash from three vessels there would be the wash from only one bow. It also possessed considerable advantages in respect to the passing through locks, as the parts could be disconnected and connected again with the utmost facility. Another advantage was that one crew was sufficient for the whole of the train, instead of a distinct crew being necessary for each boat, and at different points along the route boats could be dropped or taken on.

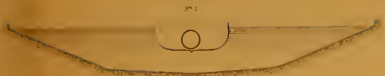
Mr. C. J. Appleby remarked, that the subject of steam traffic on canals had been much studied in Holland and in Sweden, and good results had been obtained in both those countries. Having a rather intimate knowledge of the details of working in Holland, his remarks would be limited principally to that country. There, as elsewhere, the first idea was to use a small screw-propeller boat, as a substitute for towing the ordinary cargo boats by horse power, and the working results showed a great economy over the old system of haulage. But the advantage of larger boats, each carrying its own steam power, was soon seen, the principal objection to their use being, that the driver and stoker, whose wages were comparatively high, were unemployed whilst the boat was taking in and discharging cargo. This unproductive labour made a serious item in the expenses, and necessarily enhanced the cost of the water carriage, which should always be a cheap means of transit. To meet these objections, he had designed the engines shown in Figs. 4 and 5, Plate 2, in which a simple form of steam winch was combined with the propeller engine. By this combination, not only were the engineers constantly employed, but, the loading and discharging being done much more rapidly than heretofore, the earnings of the boat were sensibly increased. He believed on nearly all the lines in Holland, these propeller boats were paying well. One proprietor began with small boats 35 feet long, with engines of 6 HP. or 8 HP. of that construction. The inconvenience attending the use of these small boats had been fully discussed, and they had long been superseded by larger boats carrying their own power. The smaller boats were now used at Rotterdam for towing purposes, shifting vessels, &c., and, as moderate prices were charged for the work, they were fully employed and paid very well. Subsequently the larger boats of 80 tons to 100 tons were found too small, and perhaps now the opposite extreme had been reached by building boats to carry from 160 tons to 200 tons. These were rather large, and there was a tendency to return to boats of about 130 tons. Four boats of that capacity were being built at the present time. They were 130 feet long, 16 feet beam, and 6 feet draught of water, and carried about 160 tons of cargo. The propeller was 3 feet in diameter, with a pitch of 7 feet. The engines were 25 HP. nominal, the pressure of steam was about 55 lbs., and the number of revolutions one hundred and twenty per minute. In a great number of voyages, extending over a year and a half, between Rotterdam and Nymwegen, a distance of about 70 miles, when the traffic was good, towing a boat of 70 tons behind, these boats ran on an average $13\frac{1}{2}$ miles an hour against the stream. The voyage was generally performed in about fourteen hours, giving an average of 5 miles an hour, and the total cost, in crew's wages, coals, oil, waste, &c., was £10. 1s. 7*d*., or about $\frac{1}{14}$th of a penny per ton per mile. That was exclusive of interest on the cost of the boat and repairs. Several of those boats were profitably employed in carrying forward the cargoes conveyed by the General Steam Navigation Company. By the facilities afforded by steam power, applied to the loading and discharging of the cargo, the boats could make two voyages between Rotterdam and Nymwegen per week. A great number of passenger boats were in use at the present time, and he believed they were all paying well. Most of them were owned by the skippers, who were doing better than in former times when horse traction was used. The speed was not less than 5 miles an hour, and the average speed was nearly 6 miles an hour. A suggestion had been made as to the best form of canal for steam traffic. He did not think the section proposed had been carried out in Holland, for the navigations there being much wider than was generally the case elsewhere, it was not necessary. But in Sweden, in the narrow parts the banks had been cut down in the way suggested, and at those parts it was compulsory by law to run at slow speed. Both in Holland and in Sweden, where the navigation was narrow, the speed was 3½ miles an hour, but the general speed was about 5 miles; and with large cargoes and moderate rates, the system was paying well. From observations made in Sweden and in Holland it was evident that steam power on canals would pay: and with a moderate pitch of screw and a moderate velocity of the propeller, no appreciable damage was done to the sides of the canal, even where, as in Holland, the banks were of very soft material. In some places the slopes were pitched, but as the stone had to be brought down the Rhine, it was an expensive process, so that it was only being done by degrees. That covering was used less on account of the wash from the propellers than to protect the banks on the breaking up of frost. Mr. Beardmore had made some remarks in favour of a design of boat and engines constructed from the joint plans of himself and Mr. Milne. The boiler appeared to be multitubular, and it was stated that for a period of five years the engines required no repairs whatever. He did not know whether that remark applied to the boiler as well, because his own experience with that kind of boiler was not encouraging. He preferred straight tubes across the fire-box, with a hand-hole to clear them out, as he had never found any trouble with them; but with straight tubes in vertical boilers of that sort, he had found a great amount of deposit around the tubes at the top of the fire box.

Mr. Beardmore remarked that one peculiarity of the tubes in the boiler to which he had alluded was that they were taper, and they could be cleaned out with the greatest facility in a very short space of time. They were 2 inches in diameter at the top, and 2⅜ inches at the bottom. The 'scows,' or small barges on the Clyde, had this class of boiler. Some in use had worked for seven years without repair, although under no scientific care. To give an idea of the economical way in which these steam vessels were used, he had seen them at work on one level of 7 miles, carrying minerals between an ironwork and Glasgow. The driver's wages were one guinea a week, and a boy attended to the steering. The cargo was quickly discharged, and two journeys were made per day. Any one who wished to investigate this subject would see how the results described by Mr. Appleby bore upon the advantages of size and length of vessel and freedom of waterway. On the Rhine and

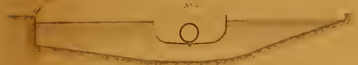
STEAM POWER ON CANALS.

TYPE OF VESSEL USED ON FORTH AND CLYDE NAVIGATION &c.

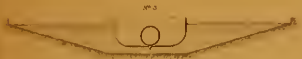
Fig. 1



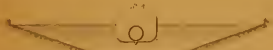
RIVER LEE OLD RIVER. TOP WATER 65 FEET. DEPTH 8 FEET.



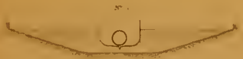
RIVER LEE IMPROVED CUTS. TOP WATER 56 FEET. DEPTH 7 FEET.



RIVER LEE OLD CUTS. TOP WATER 80 FEET. DEPTH 5 FEET (MAXM)



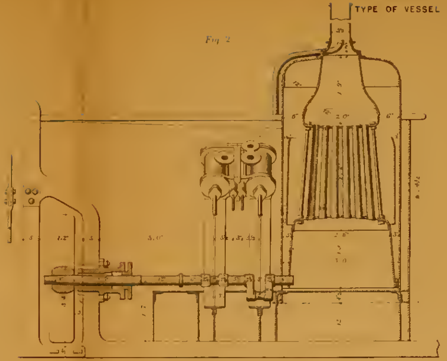
BEST INLAND CANAL TOP WATER 45 FEET. DEPTH 5 FEET.



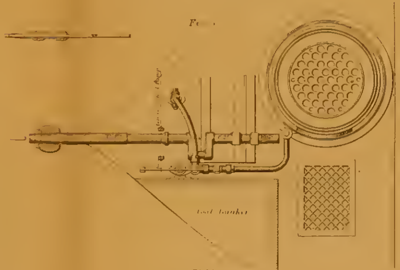
OLD INLAND CANAL. TOP WATER 40 FEET DEPTH 4 FEET

Scale

Fig. 2



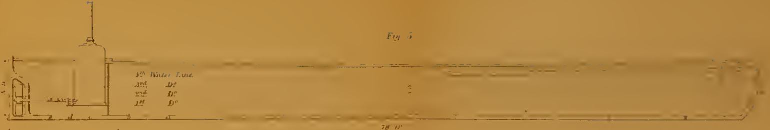
FRONT ELEVATION AND SECTION OF BOILER.



PLAN.

Scale

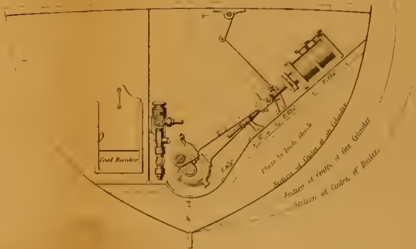
Fig. 5



SECTIONAL ELEVATION.

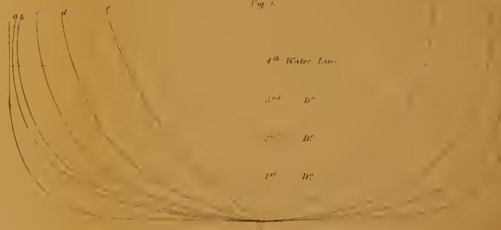
Scale.

Fig. 4


Coal Bunker
Plate to Deck check
Section at Centre of aft Cylinder
Section at Centre of fore Cylinder
Section at Centre of Boiler

SIDE ELEVATION.

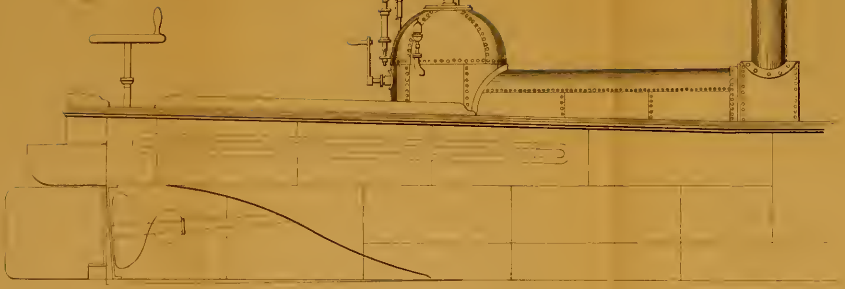
Fig. 6



Scale

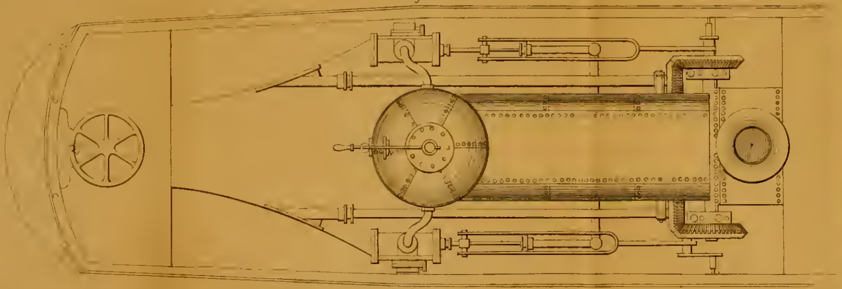
Figs 1 2 & 3 relate to the 'Pioneer' double screw
canal boat, referred to in
Mr Beardmore's remarks
4 & 5 relate to the 'Sourabaya' propeller
and as used in Holland referred to
in Mr Appleby's remarks

Fig 1



SIDE ELEVATION OF THE PIONEER

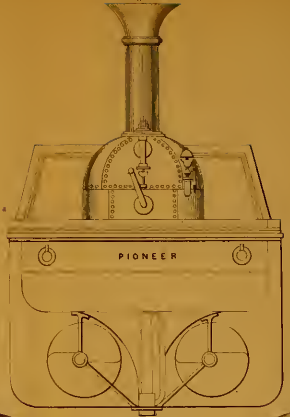
Fig 2



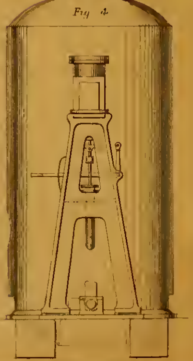
PLAN OF THE PIONEER

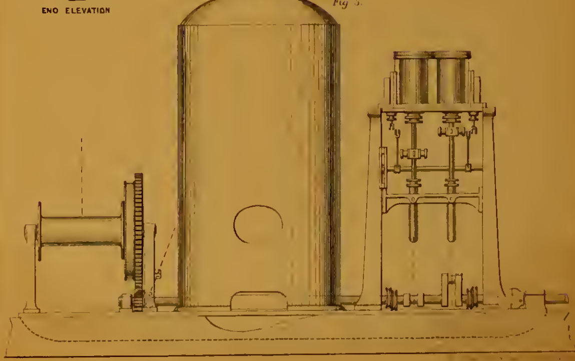
Scale

Fig 3



END ELEVATION





similar rivers, in addition to the ample sectional area, there was the power of using long craft, that described being 130 feet in length, or from 30 feet to 50 feet longer than could be used on any navigation in this country. This alone gave an immense advantage to the carrying power and velocity of the boat. In regard to the speed obtained on shallow canals he had arrived at the following conclusions:—First, that with any flat-bottomed vessel propelled by a screw, immersed to its full diameter in a canal where the sectional area of the vessel was less than one-seventh part of that of the whole waterway, the speed was sensibly affected (independently of the laws of motion of bodies through narrow canals), wherever the depth below the vessel's bottom did not exceed two-thirds of the diameter of the screw. Secondly, that when the sectional area and depth of the canal were less than the above proportions, the velocity at which the screw could be worked with advantage was limited by the speed with which the water could pass beneath the boat so as to feed the screw. In other words, if a speed were obtained beyond that at which the water would pass to the screw, the engine power was wasted in churning the bottom water.

November 20, 1866.

JOHN FOWLER, President,
in the Chair.

The discussion upon the two Papers, Nos. 1,154 and 1,165, on the "Employment of Steam Power on Canals," was continued throughout the evening, to the exclusion of any other subject.

November 27, 1866.

CHARLES HUTTON GREGORY, Vice-President, in the Chair.

No. 1,144.—“On the Smelting of Refractory Copper Ores with Wood as Fuel, in Australia.” By Joshua Llewelyn Morgan, Assoc. Inst. C.E.

The Ophir Copper Mining Company, having mines situated on the Lewis Ponds Creek, in the Bathurst District, about 140 miles west of Sydney, found themselves compelled to smelt at their mines, the ores of low per-centage that would not bear the heavy cost of inland carriage to the Port of Sydney, and subsequent charges of transit to the works at Swansea, in Glamorganshire. The district was far distant from the coal-field of Hartley (N.S.W.), but was thickly wooded, and any amount of timber for fuel could be obtained at a nominal cost.

A melting furnace had been erected there, after the fashion of those common in Peru, where however the ores are easily reduced, from their containing sufficient iron to fuse the trifling amount of silicious matter with which they are combined. The low-class ores at the Ophir Mines being very silicious, containing little iron or other matter favourable to the formation of a fluid slag, were found to be most refractory; and from an entire want of success in their treatment, the work was suspended.

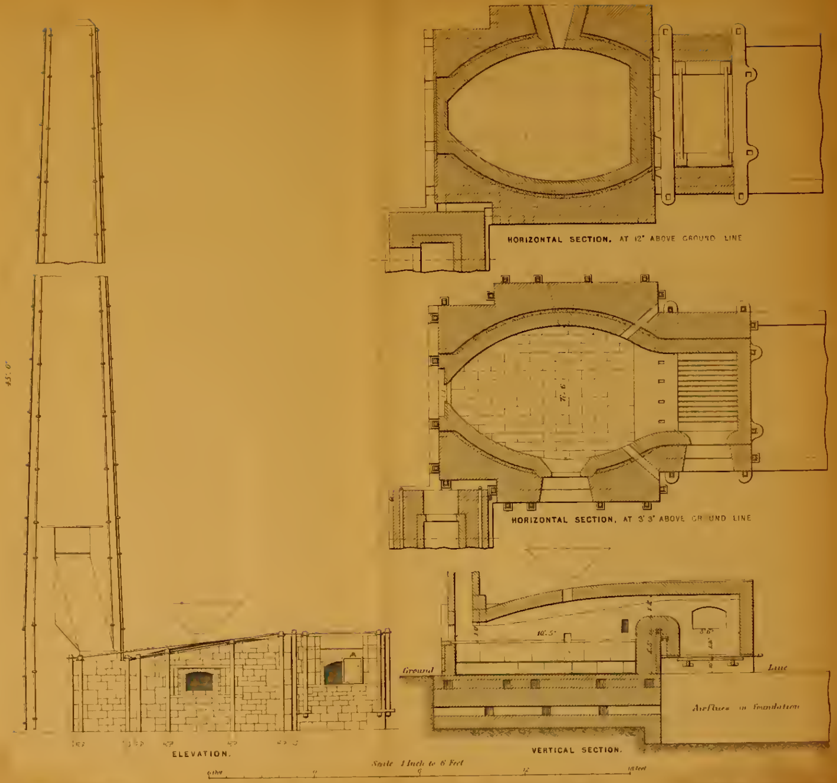
After some discussion, as to the possibility of smelting the ores with wood alone, it was decided to renew operations, under a different arrangement and with a fresh system, which led to the adoption of the measures forming the subject of the present communication. A melting furnace, of which the elevation, plans and section are shown in Plate 3, was erected; but before this was accomplished many difficulties were encountered. Fire-bricks for the roof and sides of the furnace had to be obtained; but the cost of transit from Sydney to the mines was £14 per ton, and the journey frequently occupied a bullock team six weeks when the rains set in. There were, however, on the section of land belonging to the Company, several reefs of quartz,—refractory of course, as far as fire was concerned, but not less so when any attempt was made to shape it for the bricklayer. This quartz, mined as best it could be, was put into the old furnace, kept red-hot for forty-eight hours, and then raked out into ‘boshes’ of cold water. It was now easily pounded into particles small enough to be moulded into a brick, when mixed with a proportion of clay wash. Material for the bottoms was also required. Pounded quartz would not avail here. Five miles away, a quarry had been opened on a bed of sandstone, lying in the Silurian system,—the geological feature of the district. This stone was known to endure the ordinary heat of a fire-grate hob without splintering. Fortunately it was discovered to be capable of withstanding a white heat, and was moreover, when quarried, easily dressed; and with this the false and working bottoms were made. In order, when constructing the furnace, to be certain that sufficient heat should be obtained, the area of the grate was made equal to the ordinary Swansea melting furnace, and the body of the furnace considerably smaller. These precautions proved to be unnecessary, for as soon as the peculiarities of the combustion of timber were ascertained, a more intense heat was obtained than would have been the case from first-class coal; and as no clinker was produced, the additional labour of more frequently replenishing the furnace, was less than is ordinarily expended in keeping the fire-bars of a coal furnace clear and bright. The chief peculiarity, which had not hitherto been observed, was the tendency of the cold air to form pipes, or passages, between the ends of the logs and the sides of the fire chamber, which allowed so much cold air to get into the body of the furnace, without passing through the fuel, as greatly to diminish, and occasionally almost to neutralize, the heat produced from the legitimate combustion of the fuel. To obviate this, dead plates, or shelves of cast iron, were placed round the fire chamber, at the level of the fire-bars, and projecting fully six inches into the area. There was now no chance of air passing but through the burning fuel, and the effective heat in the furnace was increased in a degree greater even than was anticipated. The ore, although subjected to this increased heat, continued nevertheless most stubborn and intractable. Limestone was added, with partial success, but the main element (iron), of a practically good slag, was still wanting. After searching in various localities, a deposit was found, evidently the back of a copper lode, and, when added in proper proportions to the charge, produced the desired effect,—a fluid slag, from which the copper separated itself without further difficulty. There was not the slightest trouble in maintaining a very intense heat; the only care necessary was to provide a good stock of wood, which should be kept long enough under cover to be perfectly free from moisture (hygrometric at least) when required for use.

The quartz bricks proved admirably suited for the purpose, as after constant use for four months the roof was in excellent order, not requiring the slightest repair. It was observed that the sides, at the bridge, were considerably worn; more so than any other portion of the furnace. This was accounted for from the diminished area of the passage, through which the flame of the grate passes to the body of the furnace, just at this point. For example, a volume of flame, or gas, rises from a grate of, say, 16 superficial feet area, is made to pass through this contracted passage of less than 6 superficial feet area, and then to expand, at the middle of the body of the furnace, to nearly 15 superficial feet area. This must be wrong; but as far as the Author is acquainted, the practice is universal in reverberatory furnaces. If the roof were so shaped as to avoid this throttling, and to diminish gradually the area from the grate to the flue, or stack, this difficulty would be avoided, and immense benefit be derived in the application of the heat.

There are other peculiarities in the construction of the furnace worth observing. The hollow bridge was so arranged, with a register attached to small flues passing upwards, as to admit such an amount of air on the under, or working, side of the flame, as would be required to combine with any excess of carbonic acid that might exist in the gas passing over, and secure its perfect combustion. The binding of the furnace, especially about the grate, proved very effective, as no crack of any kind showed itself, up to the date when the Author left the works, four months after the commencement of smelting operations.

The communication is accompanied by one drawing, from which Plate 3 has been compiled.

REVERBERATORY FURNACE,
FOR SMELTING COPPER ORE.



HORIZONTAL SECTION, AT 12" ABOVE GROUND LINE

HORIZONTAL SECTION, AT 3' 3" ABOVE GROUND LINE

ELEVATION.

VERTICAL SECTION.

Scale 1 Inch to 6 Feet

Mr. D. Forbes said, one main point in the question of cost was the quantity of fuel used in proportion to the ore smelted. He was acquainted with the furnaces used in Peru and Chili, and, as a general rule, the consumption of fuel was equal in weight to the ore smelted; that was, 100 parts of wood smelted 100 parts of ore. He had worked both cupola and reverberatory furnaces, and had found the difference in consumption of fuel even greater than had been stated. Using charcoal instead of wood, not more than one-fifth of the weight of the ore was required. Where charcoal could be obtained, a reverberatory furnace of the construction here alluded to should not be used.

Mr. C. W. Siemens agreed that there was an important omission in the Paper respecting the consumption of fuel, which was the great criterion of the goodness of a furnace. Being interested in copper-smelting in the Caucasian Mountains, he knew the difficulty of procuring materials in new countries, and considerable credit seemed to be due to the Author for having overcome those difficulties in a rational manner. The fire-brick which he had produced was analogous in composition to the Dynas brick of Swansea, which latter was composed of crushed silica mixed with lime, pressed and burnt hard, and there was no brick of greater resistance to fire than that. The furnace described in the Paper was very similar to that usually employed at Swansea for this purpose, but the fuel was different. He had seen many furnaces worked by wood as fuel. In France at several of the large glass works, such as those at Baccarat, Trèlon, and St. Louis, wood was used as fuel until latterly, and there had been no difficulty in getting the intense heat required. To obtain this heat the wood was carefully selected, and very fully dried in stoves. The method of firing was, that two firemen were always at work throwing into the furnace single pieces of wood. By that means the wood was converted in a continuous manner into a kind of gas, and a very steady heat was obtained. At present those furnaces had been entirely changed under his direction; gas was produced from undried and unsaleable wood in a separate receptacle, was heated in regenerators before reaching the furnace, and was burnt with air heated in the same manner by the wasted flame of the furnace. By that means the consumption of fuel was reduced one-half in quantity, besides being of a cheaper kind. Notwithstanding the advantages which might be obtained in this manner, he thought it questionable whether a copper-smelting furnace of the reverberatory form was right for a country like Australia. In Swansea these furnaces had a peculiar advantage, because ore was very dear and fuel very cheap, and a system was followed of concentrating the copper in the copper stone by successive meltings; that was, the slag, or waste of the second melting having imbibed some copper from the comparatively rich regulus, was made the flux of the first melting; the slag of the third melting was made the flux in the second operation, and so on. For mining countries, where the object was simply to melt the abundant ore and to produce copper stone of sufficient richness to be worth the carriage to Swansea, he believed the cupola was the right form of furnace, as it did a great deal of work with the least expenditure of fuel. Mr. Forbes had stated that in Chili the consumption of fuel was equal to the weight of ore smelted; with the cupola furnace, one-third of that quantity of wood properly charred would be sufficient, or one ton of wood would smelt three tons of ore. He believed that the form of furnace described was imported from Swansea, first to Chili, and then to Australia, although the conditions were totally different in those countries; and he felt satisfied that, for copper smelting, attention should be directed to the cupola form of furnace, as it would be found to possess great advantages.

Mr. Morgan observed, through the Secretary, that it was no doubt perfectly true that the cupola was the best method in Chili. At the Summerhill mines, in Australia, he had tried a cupola with charcoal and fan blast. He was, however, obliged to relinquish it, from the scarcity and costliness of skilled labour, and the difficulty of keeping together charcoal-burners, engine-drivers, and others of that class, even after a commencement had been made. His staff of smelters at the Ophir mines was chiefly made up of bush-men (ticket-of-leave men), who had never seen a furnace before. The comparative value of a smelting apparatus, of course, turned upon the relative quantity of fuel used in producing a certain result—other things being equal; but, under the circumstances in which he was placed, the reverberatory furnace was the only shape by which the result sought could have been obtained. Whether any credit or not was due to him for erecting such an one as did its work effectually, and profitably, when others had failed, appeared to be the question for determination.

No. 1,166.—"On Light Railways in Norway, India, and Queensland."[1] By CHARLES DOUGLAS FOX, M. Inst. C.E.

THE subject of the construction of Light Railways is one of daily increasing importance. In Great Britain, the trunk lines have been made with a view to carry an immense traffic at high speeds; and, in most instances, in a massive manner well suited to their purpose. Other countries have followed this example; and throughout Europe, first-class railways may be found, with ruling gradients of 1 in 100, minimum curves of 20 chains radius, and rails weighing from 60 lbs. to 84 lbs. per yard; worked by locomotives having from 10 tons to 16 tons on a pair of wheels, and weighing from 30 tons to 45 tons each. But, on the other hand, railways have been constructed, especially in America, to be worked by locomotives equally heavy, although with general works, and especially permanent way, unsuited for such traffic; and thus have arisen heavy working and maintenance expenses, and numerous accidents.

A demand is now arising for railway intercommunication between places not of sufficient importance to justify the cost of a first-class railway; especially in colonies and other countries, where such communication is required for the purpose of attracting population, and where, for many years, the traffic is sure to be of a comparatively light character.

By the term "Light Railways," the Author would wish to be understood such as, either being branches from existing trunk lines, or being intended for districts requiring the development of their traffic, should be constructed in a thoroughly substantial and durable manner, equal in their details as to quality to the best trunk lines, but with every part made only of such strength as to carry loads represented by the rule, that no pair of wheels should be allowed to have more than 6 tons upon it. This would enable these lines to carry the rolling stock of all other railways of similar gauge, with the exception only of the locomotives.

The first railways constructed upon this principle, which have come under the Author's notice, are those of the Norwegian Government; the designs for which were prepared, and the works carried out, under the guidance of Mr. Carl Pihl, the State Engineer. The Author visited these lines in 1864, and was struck with their efficiency and economy. With the exception of the line from Christiania to Eidsvold, which was constructed some years since by Mr. G. P. Bidder (Past-President Inst. C.E.), and the branch therefrom to the Swedish frontier—both of which are of the 4 feet 8½ inches gauge, that being the gauge adopted in Sweden—the railway system of Norway is upon the light principle, and of the 3 feet 6 inches gauge. The two lines visited by the Author may be taken as types of the system. The first, from Grundset to Hammar (Plate 4), on the Miösen Lake, a distance of 24 English miles, passes through an easy undulating country, has ruling gradients of 1 in 70, with curves of 1,000 feet radius, and has cost, including rolling stock and stations, £3,000 per mile. The second, from Throndhjem to Stören (Plate 4), a distance of 30 English miles, passes through a difficult country. The earthworks are heavy, including several rock cuttings and embankments of bad clay. There are twelve large bridges on the length of 30 miles, three of them of great height and length, the largest being the Sloppen Bridge over the River Nid. This is 620 feet in length, and has five principal spans of 70 feet each. The piers are 100 feet high, making the total height of the viaduct 110 feet. Up to high-water level the piers are of masonry, to resist the ice, but the rest of the bridge is of timber. The timber trusses are 10 feet in depth, and 11 feet apart, and are constructed upon Warren's principle. These structures are so carefully put together, that there is scarcely any vibration during the highest wind. The gradients on this line are chiefly 1 in 100. There are, however, 5 miles of 1 in 52, and in the opposite direction from the summit, 4 miles of 1 in 42, followed by 4 miles of 1 in 65 and of 1 in 100. Frequent curves are found throughout the line, but especially on the heavy gradients, where they are chiefly reverse curves, ranging from 700 feet to 1,000 feet radius. There are two terminal stations, six intermediate stations, and three stopping places, with workshops and engine and carriage sheds at Throndhjem. The total cost of the line, including rolling stock and stations, has been £6,000 per mile.

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[1] The discussion upon this Paper occupied portions of two evenings, but an abstract of the whole is given consecutively.

The permanent way upon these lines consists of flat-bottomed rails, weighing from 37 lbs. to 40 lbs. per lineal yard, fished at every 21 feet, with plates 11 inches long, and secured by dog spikes only to transverse sleepers, 2 feet 6 inches apart from centre to centre; no fang-bolts, or joint-plates being used. The sleepers are of pine, 6 feet 6 inches long, by 9 inches by 4½ inches in section, uncreosoted, and half-round, laid the round side up, and adzed, to increase the bearing of the rail, to 5 inches. An inward cant of 1 in 20 is given to the rail. The ballast occupies a space of 8 feet 6 inches wide, and 1 foot 8 inches thick, and is of good quality.

The crossings are reversible, and the switches self-acting. The permanent way, after having stood the test of several Norwegian winters, forms a very smooth road. Its repairs employ one man per mile. The general works are thoroughly substantial, the bridges and stations being of pine timber. The lines are 14 feet wide at the formation level.

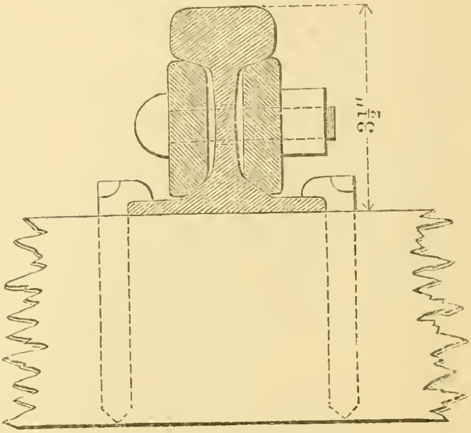
The locomotives are nearly all alike, with the exception that some have the Bissel bogie, and others Adams' radial axle boxes. They were constructed partly by Messrs. R. Stephenson and Co., and partly by the Avonside Engine Company. They have outside cylinders, 10 inches in diameter, with a length of stroke of 18 inches, and six wheels—four driving-wheels 3 feet diameter, coupled, and two leading-wheels 2 feet in diameter. They are tank engines, and are fitted for burning coal, and have a working pressure of 120 lbs. Their weight in working order is 14 tons. The train by which the Author travelled weighed, with the engine, 118 tons, and ran considerable distances at 30 miles an hour, with smoothness and steadiness. The ordinary speed required by the traffic does not, however, exceed 15 miles per hour, including stoppages. In ascending the incline of 1 in 52, an assistant engine was attached, increasing the gross load to 133 tons, or 66½ tons of gross load per engine, which was taken up with ease at about 12 miles to 15 miles per hour. The passenger carriages are 19 feet long, on four wheels, without bogies, and 6 feet 6 inches wide, by 9 feet 9 inches high outside. The goods wagons are 21 feet long, and 6 feet 3 inches wide. A single central buffer is adopted, forming also the drawbar. The under-frames are of wood.

These lines, which run through thinly-populated districts, already more than pay their expenses, and the results have been so satisfactory, that this system is being rapidly extended.

The railway from the Arconum Junction of the Madras Railway to the town of Conjeveram, 19 miles in length, and of the 3 feet 6 inches gauge, was constructed by Sir Charles Fox (M. Inst. C.E.), and Mr. G. Berkley (M. Inst. C.E.), as Engineers, and by Mr. B. Holloway, as Resident Engineer, for the Indian Tramway Company, Limited. It has now been in operation eighteen months with most satisfactory results.

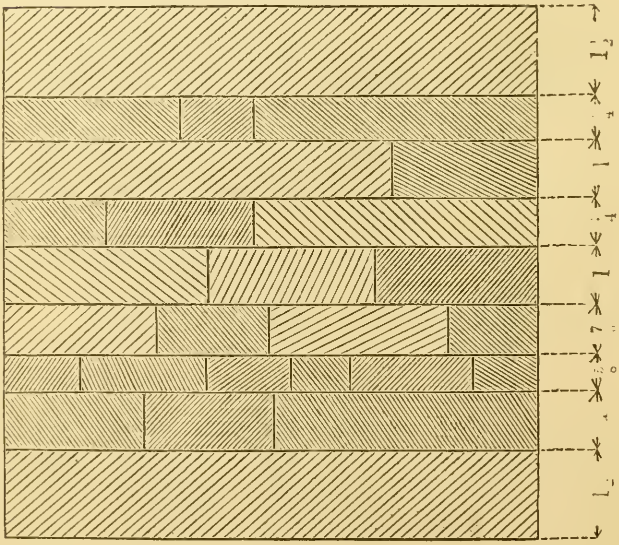
The line, which runs over a flat country, is formed chiefly of a low embankment, with frequent culverts for drainage, and with two iron bridges, on screw piles, of considerable size, to cross rivers exposed to heavy floods. The works are substantially executed. The permanent way consists of flat-bottomed rails, 35½ lbs. to the yard (Figs. 1 and 2), properly fished, and secured by dog spikes to transverse teak sleepers, 2 feet 6 inches apart from centre to centre, and well ballasted. The locomotives and other rolling stock are nearly similar to those used on the Norwegian lines, adapted however to the difference of climate. The cost of this line, which was constructed by the Company's officers, was £3,200 per mile, including telegraph and stations, or £3,900 per mile including all management and rolling stock. Although the traffic does not require a greater working speed than from 12 miles to 15 miles per hour, the trains have at times been run, with perfect safety, at upwards of 40 miles per hour, including stoppages. During the Festival Days, in May, 1866, twenty-two thousand persons travelled on the line.

Fig. 1.



Indian Tramway. Section at Joint; Rail 35½ lbs. per yard.

Fig. 2.



Indian Tramway. 9 inch pile.

The relation of the cost of working to the gross receipts does not exceed that of an ordinary line.

The Government railways of the colony of Queensland—to which Sir Charles Fox (M. Inst. C.E.) and the Author are the Consulting Engineers—form at present the most extensive system to which this principle has been applied. These lines have been laid out by Mr. A. C. FitzGibbon (M. Inst. C.E.), the Engineer-in-Chief to the Colonial Government, who, upon his arrival in the colony, found that the chief want of the country was means of intercommunication, and that it would be better supplied by constructing light, but substantial, railways, than by continuing to spend something like the same amount on roads. Careful surveys were accordingly made, and it was found, that whilst the country both above and below the Main Range of hills was of an easy character, yet to surmount this range, cut up as it is by numerous and deep ravines, would require works of no ordinary character, and such as, for a line with ordinary curves and gradients, would be simply prohibitory in cost. The matter excited much interest in the colony; and the Government having, upon the advice of their Engineer, determined upon adopting a system of light railways, with a gauge of 3 feet 6 inches, much opposition was raised, resulting in the final adoption of the Government plans.

The Southern and Western Railway of Queensland will run from the town of Ipswich to Toowoomba, a distance of 78 miles, and will then divide into two branches, one running to Dalby, a distance of 52 miles, and the other to Warwick, a distance of 62 miles, making a total of 192 miles. About 50 miles have been opened for traffic, and the remaining 142 miles are in course of construction, partly by Messrs. Peto, Brassey, and Betts, and partly by local contractors. A further length of upwards of 200 miles is under survey. The Little Liverpool incline on this line, which passes a ridge 700 feet in height, is 3 miles in length, and has ruling gradients of 1 in 50, with curves of 6 chains radius. The main incline (Plate 4), which crosses a ridge 1,400 feet high, is 16 miles in length, and has ruling gradients of 1 in 50, with frequent curves of 5 chains radius. The earthworks on this portion of the line are very heavy, with cuttings in rock, and eleven tunnels, also in rock, which have been lined with brickwork, and of which one is 27 chains in length. There are also about twenty iron bridges, with lattice girders, 12 feet apart from centre to centre, and in spans varying from 60 feet to 100 feet, with an amount of water-way almost unparalleled on a similar length of line.

One of the chief works is the bridge over the river Bremer (Plate 6), close to the Ipswich terminus, for carrying a single line of narrow-gauge railway, and a public road 25 feet wide. It has a width of

37 feet between the parapets, and consists of three spans of 150 feet each. The abutments are of stone. The piers, which are 60 feet high, consist of cast-iron cylinders 6 feet in diameter, sunk into the bed of the river, and filled with concrete. Both the main and the cross girders are of lattice construction, the former being 12 feet deep. The top flange is composed of cast iron in a tubular form, the bottom tie of Howard and Ravenhill's rolled links. The cross girders are 6 feet apart from centre to centre, 37 feet long, and 1 foot 9 inches deep. The strains upon the various parts are in all cases within the limits fixed by the Board of Trade, the rolling load being taken at $\frac{3}{4}$ of a ton per lineal foot of single line. The total weight of the superstructure is only 12 cwt. per lineal foot of the bridge, which is less than one-third the weight of bridges of similar span and width sent out to the colonies for railways of ordinary construction.

The stations on these lines are of an ornamental and somewhat extensive character. The termini at Ipswich and Toowoomba, several of the roadside stations, the engineering workshops, and the engine and carriage sheds, have been sent from England, Messrs. Fisher Brothers having been the contractors for them. They are framed in iron and wood, and covered with zinc, and are fitted with traversers, turntables, cranes, weighbridges, tanks, and pumping engines, of the same description as those used in England.

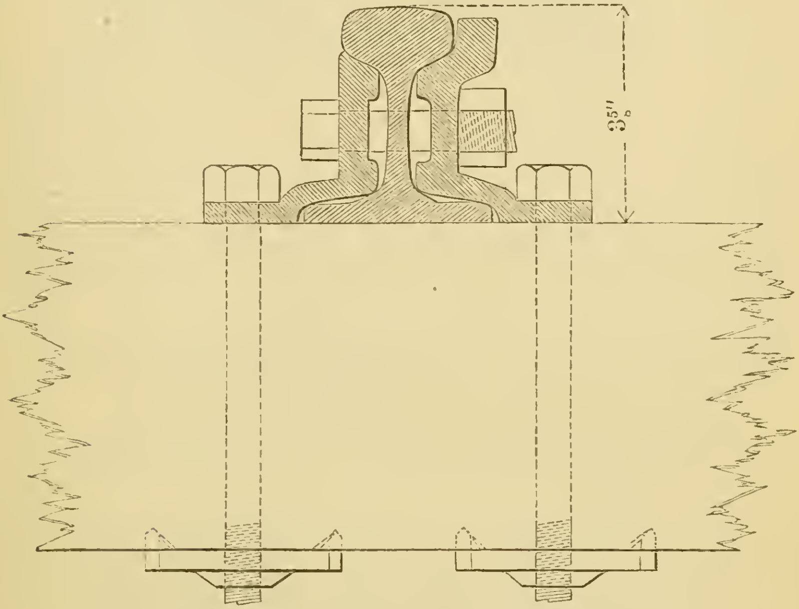
The passenger rolling stock is of the best English manufacture. The carriages are generally 32 feet long and 6 feet 6 inches wide, divided into compartments. The first-class carriages have accommodation for thirty passengers, at a cost of £17 17*s.* per passenger; the composite carriages, for thirty-six passengers, at a cost of £12 10*s.* each; and the second-class, for forty-eight passengers, at a cost of £8 10*s.* each, there being no third-class. The carriages are mounted upon iron under-frames, and are fitted partly with bogie trucks and eight wheels, and partly with Adams' radial axle-boxes and six wheels. The wagon stock varies in length from 14 feet upon four wheels to 20 feet upon six wheels, the latter being fitted with Adams' radial axle-boxes at each end. The bearing, buffing, and draw springs are of the best steel, a matter of importance, as tending towards the reduction of wear and tear. The tires, though originally of iron, are now being gradually superseded by those of steel, of comparatively light section, and fitted with Adams' springs, which have the advantage of allowing slip in passing curves, and of diminishing materially the blow between the wheel and the rail. Everything has been done, in designing this rolling stock, to keep down the weight of the various parts, so far as is consistent with strength and durability. The whole of the passenger stock, as well as one-half of the wagon stock, is fitted with Clark's continuous break, similar to that recommended by Mr. G. Berkley for the Ghât inclines on the Great Indian Peninsula Railway. The cost of this rolling stock per passenger, although fitted with every luxury, shows a considerable saving over that of other railways in sister colonies.

Three classes of locomotives are used on these railways. Tender-engines are employed upon the portions of the line where the curves and gradients are easiest. These engines weigh 15 tons in working order, and are upon six wheels, of which four are 'drivers.' The driving-wheels are 3 feet in diameter, in some cases placed behind, with the leading-wheels 2 feet in diameter, fitted with a Bissel bogie, while in others the driving-wheels are placed in front, and the trailing-wheels are fitted with Adams' radial axle-boxes. The following are some of the leading features of these engines:—Diameter of cylinders, 10 inches; length of stroke, 18 inches; heating surface, 400 superficial feet; length, 19 feet 2 inches; extreme width, 7 feet; wheel base of driving-wheels, 5 feet 6 inches. The tenders are 10 feet long, on four wheels, weigh 7 tons, have 70 cubic feet of fuel space, and tank capacity for 520 gallons of water. These engines are similar to those in use both in Norway and in India, and are found to run smoothly and well up to 40 miles per hour, though their working speed does not exceed 20 miles per hour, at which speed they are capable of taking 90 tons of gross load, including engine, upon a line having ruling gradients of 1 in 100. The cost of one of these engines with tender complete, free on board in England, is £1,400. Tender-engines are also used upon that section of the line which includes the Little Liverpool incline, 3 miles in length; but they are of a more powerful class than the former, weighing 20 tons when in working order, and upon eight wheels. The driving-wheels, six in number, are 3 feet 3 inches in diameter, placed as closely together as possible, having a wheel base of 7 feet 2 inches only, and the centre pair of wheels being without flanges. The leading-wheels are 2 feet in diameter, and are fitted with a Bissel's bogie. The diameter of the cylinders is 11 inches, the length of stroke 18 inches, the heating surface 600 superficial feet, length 21 feet, and extreme width 7 feet. The tenders are 12 feet long, on four wheels, weigh 9 tons, and have 100 cubic feet of fuel space, and tank capacity for 600 gallons of water. The motion and axles of these engines are of Bessemer steel. These engines are designed for the same gross load as those of the first-class, but for gradients of 1 in 50, though of comparatively short lengths. The cost of one of these engines with tender, free on board in England, is £1,800. The engines for use on the Main Range incline are upon Mr. Fairlie's system (Plate 5). They consist of two bogie frames, carrying a boiler, with a smoke-box at each end, and a central fire-box, having a fire surface of 71 square feet. The boiler has two hundred and seventy tubes, is 9 feet 10 inches long, with a total heating surface of 1,000 superficial feet. The frames also form the water-tank, thus keeping the centre of gravity as low as possible. Each engine has two bogies, connected with the boiler by central bogie-pins 3 inches in diameter, and by slotted quadrants having studs working in them at each end of the fire-box. These quadrants form the chief means of transmitting the power of the engine, the bogie-pins being used only as centres upon which the bogies move. Each bogie-truck is carried upon six wheels, each 3 feet in diameter, having a wheel base of 6 feet 6 inches, the centre pair of wheels being without flanges; these six wheels are driven by two cylinders, each 11 inches in diameter, with a length of stroke of 18 inches; making twelve wheels and four cylinders to each engine. The engine is provided with a fire-door on each side, and the driving platform, which is at the side, is arranged to give the driver a good view when running either way. All the gauges, working handles, reversing gear, and breaks are placed in convenient positions. The weight of the engine, including 150 cubic feet of fuel and 800 gallons of water, is about 30 tons, upon twelve wheels; and the engine is calculated to take 120 tons of gross load, at a speed of 15 miles per hour, up an incline 16 miles in length, having ruling gradients of 1 in 50, and frequent curves of 5 chains radius. The bogie-pins, quadrants, motion, valve-rods, and axles, are of Bessemer steel. The cost of each engine complete, free on board in England, is £2,500, or considerably less than if two tank-engines of half the power had been substituted. The Author has tried several experiments, on the Anglesea Central Railway, with an engine similar in construction, which weighs 24 tons, has eight driving-wheels, and takes a load of 160 tons up 1 in 70, for a distance of 1½ mile, at 20 miles per hour. This engine ran with perfect steadiness at upwards of 40 miles an hour, and the smoothness with which it passed round curves of even 170 feet radius was remarkable.

In designing these several classes of locomotives great care has been taken to keep down the weights, and it will be found that in no case does the weight on any wheel exceed 3 tons. The fire-boxes are made for burning fuel composed of two-thirds of wood and one-third of coal; ample heating surface, and fuel and water spaces, being also provided. Break power is applied to nearly all the wheels, and there is an arrangement for sanding the rails under each of the driving-wheels. The drivers are well sheltered from the heat. With a view to facilitate passing round curves of sharp radii, and for obtaining elasticity between the rail and the wheel, in none of the engines does the rigid wheel base exceed 7 feet 2 inches; all the wheels are fitted with the best springs, and with Adams' spring tires, the latter in all cases being cylindrical and not coned.

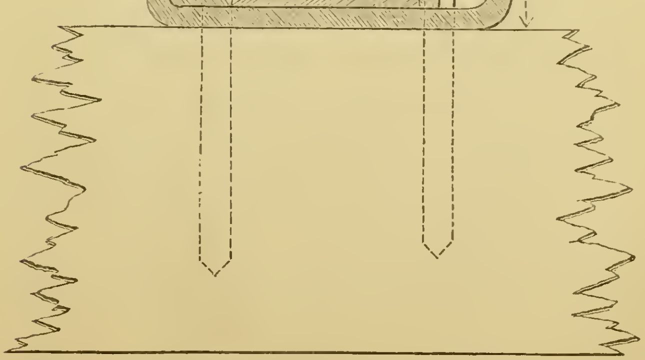
The permanent way consists of flat-bottomed rails, weighing 40 lbs. to the lineal yard, generally in lengths of 20 feet, laid vertically, fished with Adams' bracket plates, secured at the joints by fang bolts, and elsewhere by dog spikes, to transverse rectangular sleepers, laid 2 feet 6 inches apart from centre to centre. (Figs. 3, 4, and 5.)

Fig. 3.



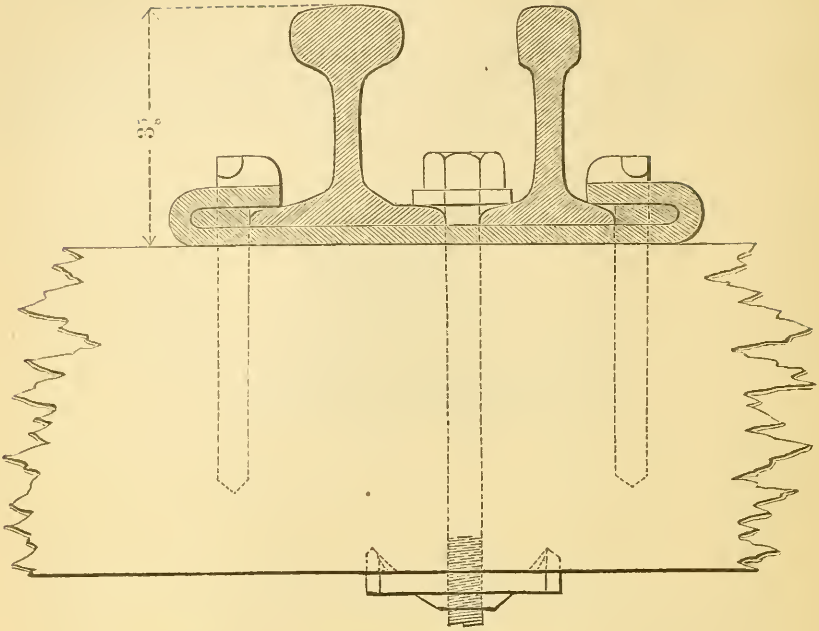
Queensland Railways. Section at Joint; Fishes 8¾ lbs. and 10¾ lbs.

Fig. 4.



Queensland Railways. Chair for Ordinary Curves: mode of fastening. Rail 40 lbs. per yard.

Fig. 5.



Queensland Railways. Chair and Guard Rail for Curves of smallest Radius.

The ballast, which is very expensive in Queensland, consists of broken rock. During the manufacture of the rails, a sample was picked out at random from each day's make, and tested upon bearings 3 feet 6 inches apart, with a dead weight of first 15 tons, and secondly 18 tons. The average deflection with the former was $\frac{3}{8}$ths of an inch, with a permanent set of $\frac{3}{16}$ths of an inch; with the latter the deflection was $1\frac{1}{4}$ inch, and the permanent set was $\frac{7}{8}$ths of an inch. The rails were also tested with a weight of 661 lbs. falling through 6 feet twice, to show a permanent set of not more than $1\frac{1}{2}$ inch at the second blow, and with the same weight falling through 9 feet once without breaking the rail. The sleepers are cut out in the forest, and at once adzed, to give accurate bearings for the rails, by a machine sent from England, which, accompanied by a portable engine, can run on the railway to the nearest point, and then, having moveable flanges on its wheels, is drawn by horses to the site where the trees are felled. In this way a great saving in haulage and in labour is effected. The cost of the permanent way, including ballast of broken stone, is £2,162 per mile, as compared with £2,996 per mile, the cost of the permanent way on the railways of a sister colony.

The cost of these railways taken from Mr. FitzGibbon's estimates is as follows:—

	Total, including Surveys, Land, Superintendence, Stations, Sidings, and Rolling Stock. Per mile.	Works only. Per mile.
	£	£
Ipswich to foot of Main Range, omitting Little Liverpool Range	7,732	6,660
Little Liverpool Range . .	12,532	11,400
Main Range	11,132	10,000
Toowoomba to Dalby . .	5,589	4,567
Toowoomba to Warwick . .	5,990	5,445
Actual average cost . .	8,600	..

From this it will be seen, that these lines may be constructed, under the most difficult circumstances, for between £11,000 and £12,000 per mile, and under ordinary circumstances for £6,000 per mile, including everything, and this notwithstanding the rates of labour ruling in the colony; being from six to seven shillings per day for an unskilled labourer, and from ten to twelve shillings per day for a skilled workman. It must also be remembered, as remarked by Mr. FitzGibbon, that "the construction of the road and the various appliances employed are in all respects equal to any railway in the world, excepting only that they are limited in power to the wants of the case."[1]

The Great Northern Railway of Queensland, which will run from Rockhampton in the north several hundred miles into the interior, and which is being carried out under the superintendence of Mr. Henry J. Plews, the Government Engineer for that part of the colony, does not at present call for any special remark. It resembles, so far as regards the first section now in course of construction, the lighter portions of the Southern and Western Railway, and is supplied with bridges, permanent way, rolling stock, engine and carriage sheds, and workshops and stations of a similar character. Its locomotives are precisely the same as those of the first-class on the Southern and Western Railway. The passenger carriages, however, which were designed by Mr. Plews, are somewhat different; being 45 feet in length, upon six wheels, fitted with Clark's radial axle-boxes, each holding twenty first-class and forty second-class passengers, the seats being placed longitudinally, on either side of the carriage, with a passage down the middle, and open platforms at the end, from which the breaks are applied. The cost of these carriages is about £10 per passenger.

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[1] Mr. FitzGibbon states, in a note to the Secretary of the Inst. C.E., dated Ipswich, 20th May, 1867, that the line to Toowoomba had then been opened, and that the 16-ton engine, four wheels coupled, took a net load of 40 tons up the Main Range incline at a speed of 12 miles an hour. The cost of the 78 miles, including everything, averaged £13,700 per mile; while the cost west of the Main Range varied from £6,000 to £7,000 per mile.—Sec. Inst. C.E.

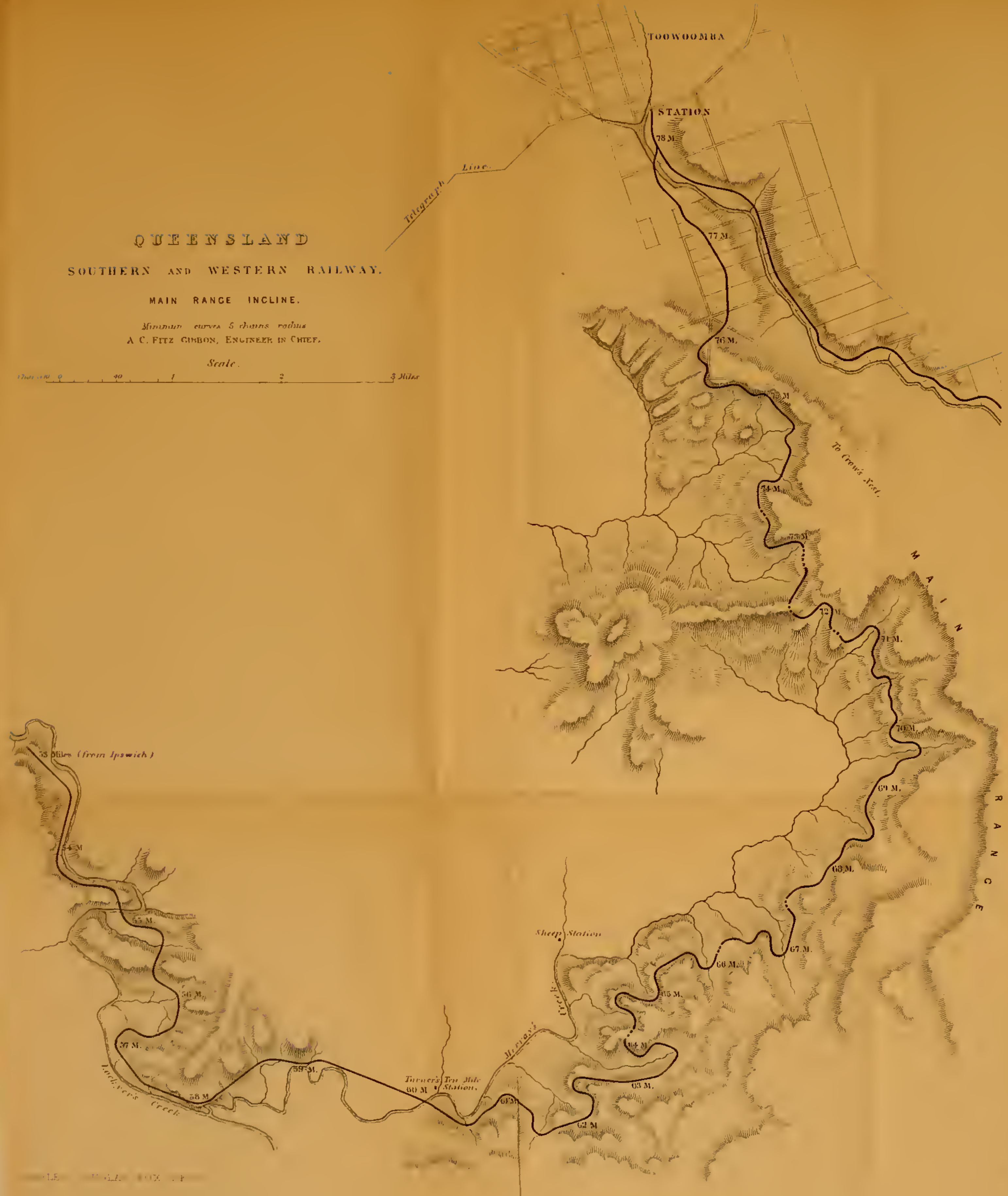
It so happens that, in the three cases referred to in this Paper, the gauge is 3 feet 6 inches. But the Author considers that the gauge is after all a matter which must be entirely dependent upon surrounding circumstances, and which, if kept within certain limits, will not affect the cost in ordinary countries to nearly so great an extent as that which, the Author would repeat, is in his opinion the basis of the light railway system—the reducing of the weight upon every wheel in the train (including the engine wheels) to 3 tons, the limitation of the speed to 25 miles per hour, and the adaptation of every detail to these conditions. In Queensland, the gauge of 3 feet 6 inches was determined on for the following reasons:—first, there were already two gauges in Australia, so that uniformity was out of the question; secondly, a curve of 5 chains radius on this gauge is nearly equal to a curve of 8 chains radius on the 4 feet 8½ inches gauge, and the country, on the Main and Little Liverpool ranges, is of so peculiar a character, with frequent ravines and spurs running out from the Main Range, that it was found, by a careful survey and estimate, if curves of 8 chains radius were adopted, as upon the Blue Mountains, New South Wales, the cost would be increased more than threefold. In the item of viaducts alone, the cost would have been £35,040 per mile, as against £6,100, an amount quite beyond the means of the colony. The gauge was therefore fixed at 3 feet 6 inches, and the minimum radius of the curves was reduced to 5 chains.

In a country, however, where the trunk lines already exist, of either the 4 feet 8½ inches, or some other gauge, and where the country through which the branch line has to pass is not of a peculiarly difficult character, requiring very sharp curves, it would, in the Author's opinion, be generally found advisable to avoid break of gauge, constructing the branch upon the light system, so that it may take all the rolling stock which the parent line may bring to it, with the one exception of the locomotives.

In conclusion, the Author trusts that this Paper may elicit a discussion which shall bring forward, more forcibly than he has been able to do, the importance of a system which he believes will be found before long to have much influence upon railway construction for branch lines, both in this and other countries, especially where the amount of traffic is somewhat limited, and where high speeds are not required.

The communication is illustrated by numerous diagrams, from which Plates 4, 5, and 6, and woodcuts Figs. 1 to 5, have been compiled. The Appendix contains a copy of the Report, dated Brisbane, Queensland, 9th July, 1863, from Mr. FitzGibbon, upon a proposed line of railway from Ipswich to Dalby and Warwick, *viâ* Toowoomba.

LIGHT RAILWAYS.



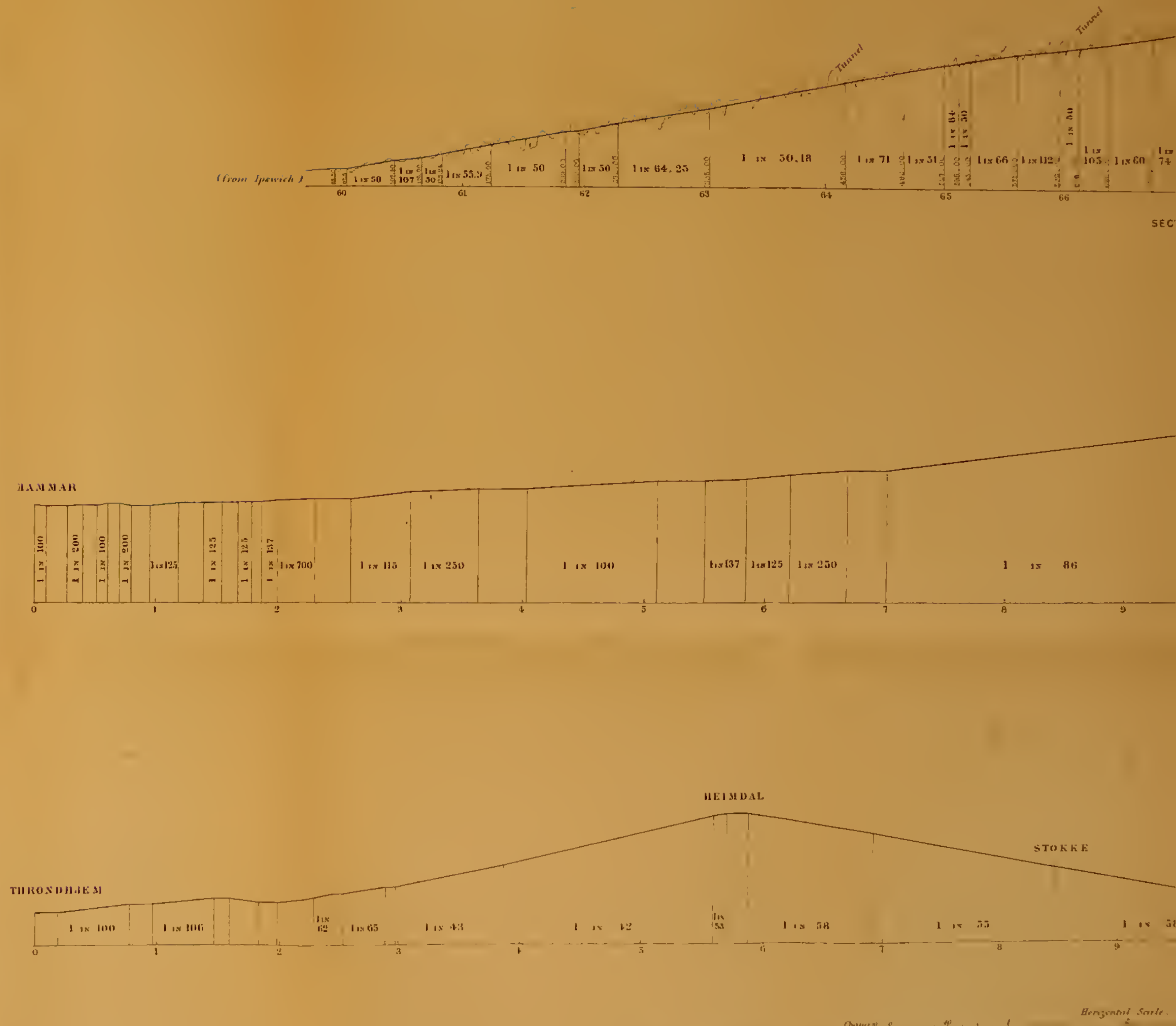
QUEENSLAND

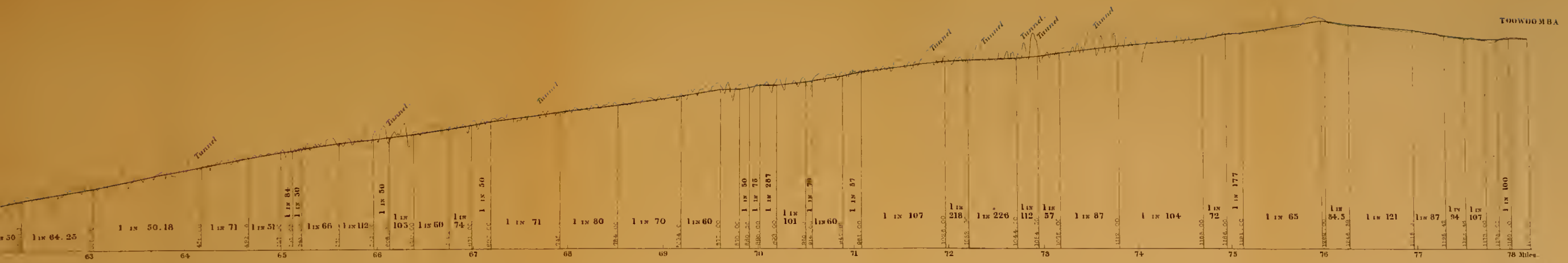
SOUTHERN AND WESTERN RAILWAY.

MAIN RANGE INCLINE.

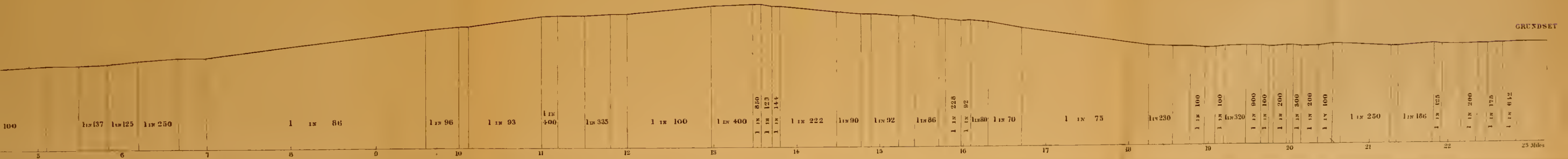
Minimum curves 5 chains radius

A. C. FITZ GIBBON, ENGINEER IN CHIEF.

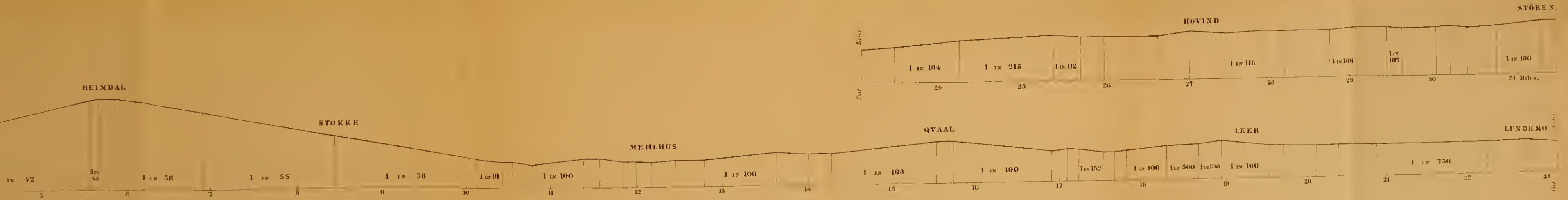




SECTION OF MAIN RANGE INCLINE, SOUTHERN AND WESTERN RAILWAY, QUEENSLAND.

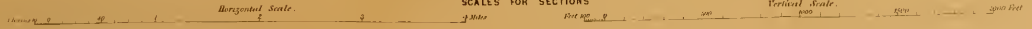


SECTION OF HAMMAR - ELVERUM RAILWAY, NORWAY.



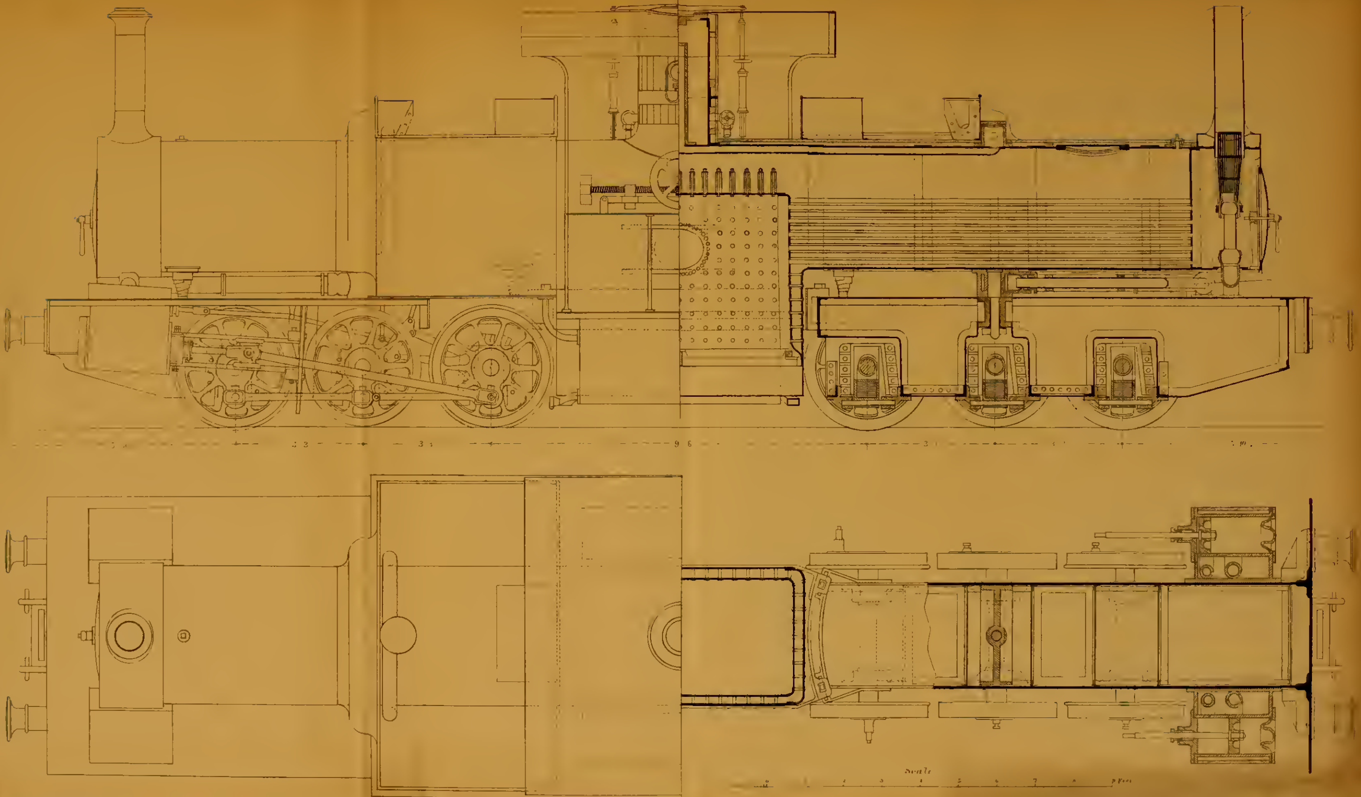
SECTION OF THRONDHJEM - STÖREN RAILWAY, NORWAY.

SCALES FOR SECTIONS



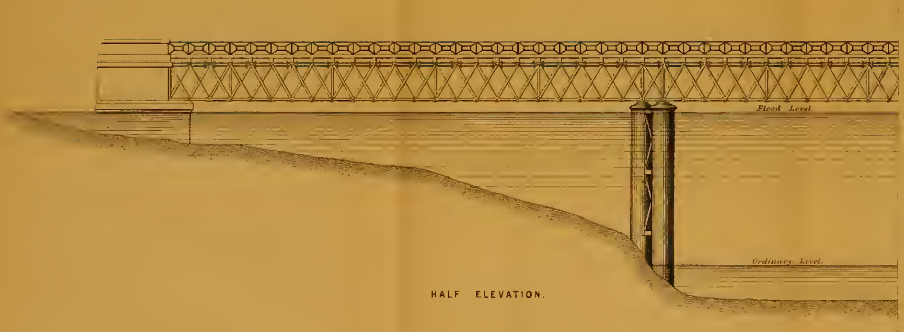
LIGHT RAILWAYS.

12_WHEELED DOUBLE BOGIE LOCOMOTIVE: FAIRLIE'S SYSTEM, FOR THE QUEENSLAND RAILWAYS



LIGHT RAILWAYS.

BRIDGE OVER THE RIVER BREMER. QUEENSLAND



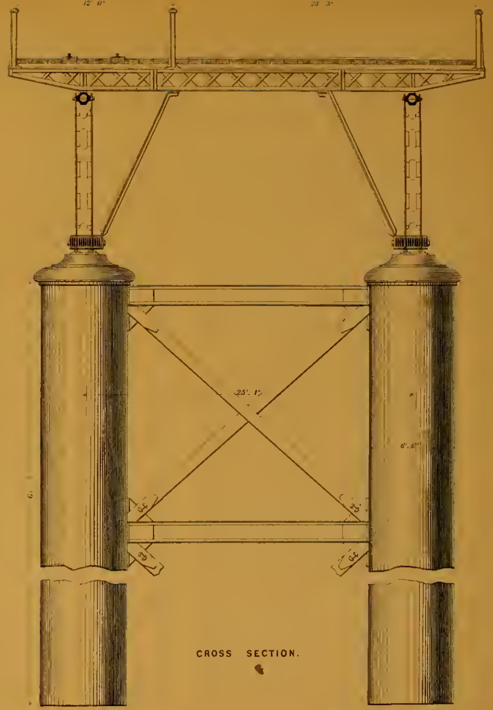
HALF ELEVATION.

Scale for Half Elevation

10 0 10 20 30 40 50 100 150 200 Feet.

Scale for Cross Section.

0 1 2 3 4 5 10 15 20 25 30 35 Feet.



CROSS SECTION.

Mr. Charles Douglas Fox said, since the Paper was written, a few facts had come to his knowledge, resulting from the system which he advocated having been at work six months longer. The system he was there to support was, that a railway should be made proportionate to the traffic which was to be carried upon it; and in order so to proportion the railway for a light traffic, it should be constructed in such a manner as to carry safely, at a speed of from 15 to 25 miles per hour, a rolling load, in no case exceeding 3 tons on each wheel. Light railways were often heard of; but many were unsuccessful, because they were not only light, but inefficient. He held, that if this system was to be efficient, it must not be started on a false principle; but by constructing the lines in a thoroughly substantial manner, so that they might be worked at a low rate. Therefore, the lines he had alluded to were constructed in the best manner, equal in all their details to the best English railways; but every part was so proportioned, as to be suitable to rolling loads represented by a weight of 3 tons only on a wheel. In laying out the Southern and Western Railway of Queensland, it became necessary to adopt unusually sharp curves and rather severe gradients, and, with the object of overcoming these difficulties, the gauge was reduced to 3 feet 6 inches, so as to allow of the use of curves of 5 chains radius; and in the selection of the rolling stock, all had been done as far as possible to avoid the evils resulting from such sharp radii. Several ingenious plans had been adopted for avoiding those difficulties, but the one to which the chief trial had been given was Adams's radial axle system. That had been applied to carriages with six wheels, the centre wheels being fixed in the ordinary way between horn plates. The traffic manager of the West Hartlepool Railway had found it answer admirably. At first it appeared necessary to have four wheels in the centre of the carriage to steady it, but experiment proved it was not so; and even when a carriage was at the end of the train, there was no unusual swaying movement. He was informed that the trains never ran more smoothly than when fitted with the radial axle boxes, and that there was a large decrease in the tractive force necessary. Experiments were also being made with wheels loose on the axle. That plan had been tried by the late Mr. Brunel, although he believed it did not meet with success; but it would be a great desideratum in this case for the four-wheel carriages, if it could be brought into practical working. It would be seen that a long bearing of 12 inches had been provided, the wheel being 24 inches in diameter, with provision for tightening up the bearings and for thorough lubrication. As materially affecting the question of cost, he directed attention to two specimens of permanent way,—one composed of rails weighing 84 lbs. to the yard, for engines having a load of

7 tons on a wheel, the other, a sample of the rails laid on the Queensland line of 40 lbs. to the yard, suited for a weight of 3 tons on a wheel—the one being adapted for heavy engines and a speed of 40 or 50 miles an hour, and the other equally well adapted to light engines and a speed of 20 or 25 miles an hour. If the railway system was to be extended, not merely to the highways but to the byways of traffic, it would be necessary to find out some means of reducing the cost of construction. The lines in England had cost from £29,000 to £33,000 per mile; those in France about £27,000 per mile. The railways in Ireland had cost about £15,000 per mile, and those in India about the same. It was difficult to make a satisfactory comparison, because some were double lines and others single lines, and in many cases the prices of land varied considerably; but in Queensland, where the rates of wages were excessively high, the average cost of the line, as had been stated in the Paper, was only £8,600 per mile. The Norwegian lines had cost about £6,000 per mile, through the more difficult country, and over the ordinary country, only £3,000. The line constructed by the Indian Tramway Company had been made at an average cost of £3,200, or, including rolling stock for a large traffic, £3,900 per mile, the land being provided by the Government, and the earthworks being generally very light. He wished to add that he was not here speaking of lines in any way inefficient. He believed these lines were as well constructed as any that could be found. The heavy bridges on the Queensland line were constructed of iron, as also the stations, the latter, which were thoroughly substantial, being covered with corrugated zinc. The station buildings were first temporarily erected in England, then taken down and the parts marked, so that there might be no difficulty in fitting them when sent out. The reason why that description of station had been hitherto adopted for Queensland, was on account of the great difficulty of getting skilled handicraftsmen. It had, therefore, been thought better to construct an iron station in this country, take it down again and send it out, than to build masonry or brick stations in the colony. Hitherto the working expenses on these lines had not exceeded 45 per cent. The engines weighed about 15 tons each, and the cost per train mile was $8\frac{1}{4}d.$ for fuel, oil, tallow, and wages. The axle-boxes, which were for oil upon Garr's patent, answered admirably, and the consumption of oil for four wheels was 8 gallons per 40,000 miles run, or 2 gallons for each wheel. Light railways had been constructed in America, but had yielded different results, inasmuch as it was a common practice to make the works light, and then run heavy engines over them; and that was not the way to get good results. This question had now become an important one in this country. There were many places in England, Ireland, and Scotland in great need of railways, which could not

bear the cost of an ordinary line; but if they could be made at from £4,000 to £5,000 per mile, including everything, no doubt there would soon be a rapid extension of the railway system. The Board of Trade had been alive to this system; but the difficulty was, how to prevent heavy engines running on the light lines unless there was a break of gauge. This brought him to the question of gauge. He had described three lines, each having a gauge of 3 feet 6 inches, but that was not the principle he advocated, but putting light weights on light rails. The gauge on these lines had been adopted from a variety of adventitious circumstances. The question of gauge was one which must always be determined by the surrounding circumstances. He did not think the cost of railways, through an ordinary country, would be found to depend so much on the gauge as on the way the lines were constructed. This principle was now being largely tried. The Queensland Government would probably spend two millions and a half on railways within a few years, and it was already constructing 250 miles. The Norwegian Government had completed about 60 miles, and was carrying on further works, whilst the Indian Tramway Company had about 20 miles in working order with satisfactory results; and he had been surprised at the intelligence received from the resident Engineer, that trains had been run at the rate of 40 miles an hour, including stoppages, which, with driving-wheels of only 3 feet diameter, must be regarded as a very high speed.

Mr. G. W. Hemans said, it seemed at the first view that the reduction of the gauge of a railway from 4 feet 8½ inches, to 3 feet 6 inches, being only a difference of 14½ inches, could not possibly result in such an enormous saving as that which had been mentioned. It was stated that an efficient railway could be made complete, and doing all the work as well as a more costly one, for £3,000 per mile, including stations and rolling stock. That no doubt was a solitary instance, but other facts were given which brought out the cost of these narrow-gauge light railways, constructed under the most formidable difficulties, only as high as £12,500 per mile, with long viaducts of great height and heavy works of all kinds. The present period might be regarded as peculiarly the time for railway Engineers to reform their operations, looking at the existing want of confidence on the part of the public in railways as a paying investment. In reference to these cheap lines, it was true the alteration which had brought about such remarkable results was not confined solely to the alteration of the gauge, and no one could doubt that an important step had been taken in the direction of getting locomotive engines which would produce all the results required, without the enormous and ruinous weights upon the driving-wheels, which were now destroying the permanent way of many railways. There had been many discussions on permanent way, steel rails, &c., but the permanent way in point of form was almost what it was twenty years ago; the only great improvements were the fishing of the joints of the rails, the strengthening of the permanent way by adding to the weight and dimensions of every part, and the introduction of steel rails. But this had been done at a cost which produced absence of dividends. In this country only three or four important lines were paying good dividends. Some improvement must be introduced if the public were ever again to bear a fair share in railway speculation. If railway enterprise ceased, the country must suffer; if they could not put guarantees upon the counties and baronies to support the railway system, then there must be found means to produce renewed confidence. In the days of the stage-coach traffic, the weight of passengers, &c., carried was considerably greater than that of the vehicle, whilst in the case of railways the weight of the vehicles was four or five times greater than that of the loads carried. Therefore the suggestions made in this Paper, as to the improvement of engines and rolling stock, were very important. Ever since he had heard of light railways he had been endeavouring to ascertain how to reduce the weight upon the wheels of the engine, and the wear and tear of the permanent way, by the use of lighter rolling stock with steel frames, and by the introduction of bogie frames to diminish the friction on curves; and he had considered whether such light railways as those of Queensland could not be introduced into districts which would not repay the cost of the construction of railways on the ordinary system in use in this country. But he was anxious to hear the views of other Engineers, as he had not in his own conclusions arrived at any means of adopting the light railway system, except in so far as the reducing of the weight of the locomotive and the rolling stock. If a light railway of the usual gauge was adopted, with a road and bridges of strength sufficient to carry only 3 tons on each of the wheels, instead of 6 or 8 tons, what was to insure that, under peculiar pressure of the traffic, the heavy locomotives and existing heavy rolling stock would not be run upon the light railway and destroy it. A light railway of the usual gauge in connection with any other system, would always be open to the incursions of the heavy rolling stock, and for that reason break of gauge must be contemplated if the light railway be used. By employing a 3 feet 6 inches gauge, smaller tunnels, lighter cuttings, and lighter bridges sufficed; very little ballast was required, and altogether the cost of construction was greatly diminished. If a break of gauge were determined upon, a considerable saving in construction would be effected, which might give the proprietors a dividend, where otherwise there would be none; but it must not be forgotten that there were serious objections to a break of gauge, and often those objections were insuperable. He thought the question was one of the deepest importance; but as far as he was personally concerned, he had only in one instance been able conscientiously to recommend the adoption of this new system, and that was in the Isle of Man, where there could not possibly be any communication with other lines. Under such conditions he should be induced to try this system, by which a probable saving of £20,000 in ten miles of line might be effected. But, unless the circumstances of a country were such as to make it worth while to construct an isolated railway that would not carry the stock of the main lines, he thought it would be impossible to follow the suggestions in the Paper, except in the important particular that endeavours should be made as much as possible to diminish the enormous and ruinous weight of the present locomotive engine. As far as he could judge, this had been judiciously done on the Queensland Railway, and he thought the attention of railway engineers should be directed to the consideration of such engines as Mr. Fairlie's, or the inventions of Mr. Adams, by which additional adhesion was obtained by moveable wheels, and by that means reduce the enormous destruction of permanent way now going on from the action of the heavy locomotives.

Mr. G. B. Bruce could not quite agree in the presumed necessity for introducing these light railways into England. His own impression was, that there was really very little difference in the cost between making a line which would carry moderately heavy rolling stock and making a line to carry very light stock. The over bridges would be the same, but the under bridges might perhaps be a little lighter, and to that extent there would be a saving. The permanent way again would be lighter, and that would save something; but when it came to the question of stations, nothing really was saved, because whatever might be the gauge of the railway, the stations must be in accordance with the traffic which had to be carried; and if a comparison were made between two railways, and it was said the stations relatively only cost so much, it conveyed no definite impression to the mind, because the number and extent of the stations depended upon the traffic to be accommodated, which might be very different in the two cases. The other elements in the case, such as land, signals, and all the apparatus connected with a railway must be the same, and he believed the same would apply to the rolling stock, viz., that it would cost more or less in proportion to the number of tons to be carried and the number of passengers to be conveyed, whether the stock was light or heavy. Therefore in all that constituted the capital of the line, he thought the saving was little or nothing when the amount of passengers and goods which had to be carried were taken into consideration. With regard to the adoption of a narrower gauge, the question had been in a great measure answered by Mr. Fox, who said he did not insist upon that gauge, but it was rather dependent on the amount of load brought upon the permanent way. He could not help thinking the Author was reverting to the weights of engines and carriages used twenty-five years ago rather than suggesting anything new; and so also with respect to the specimen rails exhibited; one was the 84 lb. rail of to-day, and the other the 40 lb. rail of twenty-five or thirty years ago. The question was rather one for the traffic-manager—how loads might be conveyed most economically so as to produce the best dividends. It would require a good deal of evidence to show that goods and passengers could be carried as cheaply with a small engine as with a large one. And when a little more iron was put into the permanent way, there was no doubt but that goods and passengers would be carried at less cost than on the lighter road. The saving in the cost of bridges he did not think would be much, even if the bridges were made to carry only $\frac{3}{4}$ of a ton to the foot instead of $1\frac{1}{2}$ ton as required by the Board of Trade. True, the top and bottom members of the girders would be lighter; but the additional amount of iron required to enable the bridges to bear heavier engines was so small, that he considered it would be bad economy to make bridges so as just to carry the light engines and no more. The only novelty with regard to the locomotive was in the employment of the Fairlie engine, in which there were virtually two engines fastened together and working as one engine. He said nothing about the merits of that, excepting that it had the advantage of saving a driver. Ordinarily, when two engines were worked together a driver and fireman were required for each engine; but in this case one set of men worked in the middle. He had no doubt that plan was a good one for steep inclines, and where the engine could be kept at this duty alone; but at the same time it was a good thing, in the majority of cases, to be able to separate the engines and work the two separately, and he doubted whether that engine was applicable except for working up steep inclines in special localities. He thought the idea of introducing a narrower gauge than 4 feet $8\frac{1}{2}$ inches was a mistake. There would be little saving in cost of construction, and little or nothing in rolling stock, because the engines might be just as light as the engines on the 3 feet 6 inches gauge if desired; therefore he thought it would be well to hesitate before introducing the system elsewhere.

Mr. Blair submitted that the expense and construction of a first-class railway, with a single line of way, had been greatly overstated. Mr. Fox had represented that the cost of English railways had been about £30,000 a mile, and, by way of comparison, represented the colonial railway referred to in the Paper as costing only one-fifth of that sum, or thereabouts. Now, under ordinary circumstances, for lines executed through poor districts, such as the north-west of Ireland and parts of Scotland, £6,000 per mile had been found ample for every expense of construction, including commodious stations, extensive sidings, complete signals, and suitable appliances for the traffic being conducted in an economical manner. As an instance of this, he mentioned the Great Northern and Western (of Ireland) Railway of about 85 miles in length, the cost of which, including every preliminary and executive expense, had been only £6,000 a mile. That railway had superior fencing, numerous road-bridges, and other works that in a new country might be dispensed with.

Mr. Edward Woods thought the Author was right in principle, and it was satisfactory to find, that the system had been worked out on so large a scale, with such economical results. It had been his own lot to adopt the converse of that principle, not so much to originate the construction of light railways as to adapt rolling stock to that class of railways. He had, in fact, for many years past adopted the principle Mr. Fox had enunciated, viz., that of fixing a limit to the weight to be placed upon the driving-wheels of the engine, such limit having reference to the light rails in use. The first case Mr. Woods had to deal with was a line of the ordinary English gauge, the Copiapo Extension Railway, originally intended for horse traffic, but for which that mode of propulsion was found to be inapplicable. He had to design a locomotive to run without injuring the rails, which weighed 42 lbs. to the yard. Locomotives of 32 tons weight had worked upon that line for six years, and the road was now in as good order, with regard to the rails, as it was after the first six months' working: but though the engines were of 32 tons weight, the weight was subdivided, so that not more than from seven to eight tons rested on each pair of wheels. In the case of two other railways in Chili, he had to deal with the rolling stock in the same way. He exhibited a photograph of an engine of a class which he was sending out to that country for the Tongoi Railway, a line of about 40 miles in length, with curves of 187 feet radius, gradients of 1 in 19 for four or five miles, and a gauge of 3 feet 6 inches. The gauge of the other line, the Carrizal Railway, was 4 feet 2 inches, and the same class of engine was working satisfactorily upon it. That engine weighed only 15 tons, with six wheels, each 30 inches diameter, and all coupled, having inside cylinders 12 inches in diameter, with a length of stroke of 17 inches. To adapt it to the sharp curves on the line, the leading and trailing axles were fitted with the translation system of Caillet, which worked very satisfactorily. In the first case named, that of the Copiapo Extension, he had adopted the "bogie" system, but there was in it the disadvantage that the adhesion of the bogie wheels was not available for traction. With regard to the engines of Mr. Fairlie, they were virtually a couple of engines combined. He agreed with Mr. Bruce in preferring two uncombined engines, working together if need be. He thought, where there were steep gradients to deal with, it was not an advantage to have too heavy trains, and such long double engines were inconvenient in the stations, and required large and expensive turntables. They might perhaps be well adapted for special purposes, such as that to which Mr. Fox had applied them. In working steep inclines like those in Chili, accidents sometimes occurred, the chances of which were diminished when the load was divided, and the evil consequences of a casualty were much lessened. With reference to the question of the increasing weight of rolling stock, Mr. Bruce had remarked upon the engines of twenty-five years ago, which were about the weight of those described in the Paper. He remembered the time when, on the Liverpool and Manchester line, an engine of 10 tons was thought a heavy one, and 12 tons very heavy. The rails at that time weighed only 35 lbs. to the yard, and with engines of 12 or 14 tons, those rails stood well enough; but as the traffic increased, engines of greater size and weight were introduced, and the consequence was, the rails were soon destroyed, and the line had to be relaid with rails of 50 lbs. to the yard; but the gradual introduction of larger engines rendered those heavier rails useless, and they were replaced by others weighing 60 lbs. to the yard. The Grand Junction Railway works were at that time commencing, and Mr. Locke adopted a rail of 62 lbs., which was then considered very strong. That answered well with the Grand Junction engines for several years; but the increased weight of those engines eventually rendered the rails unserviceable, and now the rails weighed 80 lbs. to the yard. He thought if the principle advocated by Mr. Fox had been earlier considered and adopted in this country, that to a certain extent much of the cost of altering lines might have been avoided. As an engine of 32 tons was made applicable to 40-lb. rails, so he thought engines of sufficient power to meet the demands of a growing traffic might have been devised for the Liverpool and Manchester, Grand Junction, and other lines, which would have allowed the rails to last double the period they did. It not only affected the question of the permanent way, but also the structures which carried the permanent way. In the year 1844, Mr. Hawkshaw and himself conjointly constructed a viaduct a furlong in length, connecting the Liverpool and Manchester and the Leeds and Manchester railways, at the part which adjoined the Bolton and Manchester line in Salford. That viaduct was constructed of cast-iron columns and girders. Three or four years ago, looking to the greatly-increased weight of the engines of the London and North Western Railway, they came to the conclusion to call the attention of the Directors to the fact, that as that structure had been designed and executed at a time when the engines were 30 or 40 per cent. lighter than they were now, it might be desirable that it should be looked into, and their joint representations resulted in the strengthening of the whole structure. Similar costly alterations of works had been carried out, and were now going on in other parts of England. He had found, whenever the Government inspectors came to pass a railway, that increased strength of rails and fastenings was demanded. What was admissible a year or two ago was not so now, and this had been the tendency for the last ten or fifteen years, and where it would stop he could not tell; but the subject was one that deserved the greatest consideration of all the members of the profession in order that, if possible, some limit might be assigned to this constantly-increasing expense.[1]

Mr. HODGE remarked that, in the early days of railways, he had

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[1] The following additional information has been handed to the Secretary by Mr. Woods:—

TONGOI RAILWAY, CHILI.

Gauge 3 feet 6 inches.

Including extension of 10¼ miles now being made is 41¼ miles in length.

Forms outlet to the rich copper mines of Tamaya. The extension commences at the foot of a range of hills, and to avoid deep ravines it winds much, and has curves of 187 feet radius. There are from twenty-five to thirty curves of small radius.

The maximum gradient on the extension is 1 in 19, and there are many steep gradients.

Rails.—42 lbs. per yard, in lengths of 18 feet to 20 feet, fish-jointed, flanged, and laid on transverse sleepers of wood.

Wagons.—Platform wagons 7 feet 6 inches by 5 feet 10 inches outside; sides, 13 inches deep, one side falls; wheel centres, 5 feet.

1*st Class & 2nd Class Carriages.*—16 feet by 6 feet outside; longitudinal seats for twelve passengers.

Engine.—Six-wheeled coupled tank engines. Cylinders (inside) 12 inches by 17 inches; wheels, 30 inches, leading and trailing, fitted with Caillet's translation apparatus; wheel base, 10 feet 9 inches; grate, 2 feet 8 inches by 2 feet 2 inches.

CARRIZAL RAILWAY, CHILI.

Gauge 4 feet 2 inches.

		Feet.
Length of old line	18 miles	Rises 734
Do. extension	4½ „	„ 826
	22½ miles.	Total rise 1,560

Average gradient over first 18 miles	1 in 129.
Maximum do. over do.	1 in 88.
Average gradient over extension	1 in 28.
Maximum ditto ditto	1 in 25.

Curves.—373 feet radius and upwards.

Rails.—44 lbs. per yard, in 21-feet lengths, fished-flanged.

Sleepers.—Ten to each pair of rails. Dimensions 8 ft. × 6½ in. × 4 in.

Wagons.—Some are on 8 wheels, and weigh 6,000 lbs.
Some are on 4 do. do. 2,500 „
100 tons for weight of wagons, represent 63 tons cargo, or net load.

considerable experience in making light roads, especially in the United States of America, and had tried various kinds of flat and edge rails of variable weights. In a country like Australia, cheap railways were essential, involving curves of short radius, and steep gradients; but, looking at the nature of the traffic, he did not think this description of gauge suitable for English railways, excepting in cases like the Festiniog line; and even there he believed a gauge of 4 feet 8½ inches might have been adopted with advantage, as the cost would not have been much greater, and the disadvantages of so narrow a gauge were considerable. With regard to the question of light and heavy engines, they had been gradually increased in weight from 10 tons up to 30 and 40 tons, to meet the enormous increase of traffic on the main lines, but with such heavy engines the wear and tear of the rails was excessive, and he should prefer dividing the weight of the trains. He had been told, by the late Mr. George Stephenson, that the loose wheel on the axle was the plan first adopted on colliery tramways; it was, however, difficult to keep grit and dirt out of the bearings. He had great experience in constructing engines with bogie frames; in fact, the bogie in its present form, was the invention of Mr. David Matthews and himself, and he had introduced, thirty years ago, engines fitted with bogies on the Patterson and New York Railway. Mr. W. Adams had recently effected some improvements, which enabled engines to traverse the sharpest curves of the North London Railway at great speed, the accommodation being in every way satisfactory, from the peculiar arrangement of the centre pin. He saw no particular objection to Mr. Fairlie's engine, though he thought it had better be divided; there was nothing new in the arrangement of the driving-axles. He thought that much credit was due to Mr. Fox, for having called the attention of young Engineers, not to what had been done in England, but to what could be done abroad, where in all probability their talents must be applied for the future. The suggestion had been made to throw a portion of the weight of the tender on the engine; this had been his own practice in America, where he had placed the driving-wheels at the back of the furnace, in a manner similar to that subsequently patented in this country.

Mr. F. W. Sheilds thought the material point which had been

Engines.—Six-wheeled coupled tank engines. Cylinders (inside), 12 in. × 17 in.; wheels, 30 in., leading and trailing, fitted with Caillet's translation apparatus; weight in working order, 14½ to 15 tons; tank 500 gallons.

The engines will take a gross load of 125 tons, representing 79 tons of cargo, up the first 18 miles, at an average speed of 9 miles per hour, and they will take a gross load of 52 tons, representing 32 tons of cargo up the 4½ miles extension, at about 5 miles per hour.

raised was as to the cost of light railways in comparison with those ordinarily in use in this country. He agreed that the permanent way, stations, and other matters, must remain pretty much alike on either system; and he considered the only way to account for the great economy in the cost of the Queensland railways must have consisted in the reduction of earthworks and masonry, caused by the adoption of sharp curves and steep gradients throughout the line. If curves of 5 chains radius and gradients of 1 in 50 were used, a large amount of cutting and embankment would of course be saved: in fact, by that means the earthworks would be reduced to a minimum. Taking the cost of earthworks at 20 per cent., and the bridges at 15 to 20 per cent. of the whole expenditure, there would be a margin of 35 to 40 per cent. on which a great saving could be made; and in earthworks alone he dare say three-fourths of the ordinary cost would be saved. In short, the reduction of the gauge admitted of the curves being decreased, and that seemed to him the chief point in which this system was productive of economy. With regard to the advantages of this system in Australia, as compared with England, he would say a few words. He had laid out lines in Australia, and had found a remarkable difference in the two countries. Australia was a new country, with few other lines of communication of any kind; whereas in England provision must be made at every step for passing other railways, roads, canals, &c., either over or under the line, involving cuttings, embankments, and other heavy works, which could not be obviated by the adoption of any curves or gradients whatever. In Australia, therefore, the benefit of the economy resulting from the use of sharper curves, allowable on the system under discussion, could be fully realised; and it seemed to him that this system could be applied with greater advantage in Australia than in England. It occurred to him also to remark, that in his own experience in Australia he made it a rule to use the materials of the country as far as he could; still he had no doubt the Author was justified in sending many things from England for the reason, that whereas the wages of workmen and other expenses were at one time moderate, they had since increased enormously. Mechanics' wages had, he understood, risen to 10*s.*, 15*s.*, and even at times 20*s.* per day; and, if that state of things prevailed, it made a serious difference in the cost of work done on the spot, and justified a considerable modification of the former practice. The timber of Australia was amongst the best in the world. He had never seen any woods which exceeded in strength and durability those of New South Wales; and he should be inclined to use them in construction as far as possible. He would suggest that Mr. Fox should be asked what was the average amount of earthwork per mile on the Southern and Western Railway of Queensland, in order to enable an estimate to be made, how far its low cost per mile was due to the circumstances he had endeavoured to explain.

Mr. F. J. Bramwell had only one observation to make, and that was in reference to the locomotive used on the Queensland Railways. It had been said that the sole advantage of such a double-ended or compound engine was, that one set of men attended to that which was in fact two engines. Mr. Bramwell had a recollection of a Paper read at this Institution, in which there was a description of the arrangement of engines used for working the steep incline on the line leading from Genoa to the interior (the Giovi incline),[1] and which engines were, he believed, designed by the late Mr. R. Stephenson, or under his advice. Two separate engines were used to draw the train, but they were coupled up foot-plate to foot-plate, so that one driver and one fire-man were sufficient for the two. He had seen those engines in operation when he travelled over the incline a few years since, and they appeared to be very efficient, and undoubtedly they had the advantages attendant on the ability to work them either coupled or separate.

Mr. Abernethy had travelled on that line recently, and he then noticed that the engines were not placed foot-plate to foot-plate.

Mr. Hemans could state that they were so placed for years.

Capt. H. W. Tyler said, when he was on the Giovi incline, in June, 1866, the engines were worked foot-plate to foot-plate. He thought there was one important saving which had not been referred to, but which had been effected by adopting a narrower gauge in certain localities, namely, in the avoidance of heavy works in mountainous countries. In flat countries, very little saving could be effected by reducing the gauge from 4 feet 8½ inches to 3 feet 6 inches. But in mountainous districts, sharper curves, which worked more easily on the narrower gauge, could be better employed. By climbing round the sides of hills instead of passing through them, an enormous amount of rock cutting, of embankment, of viaduct, and even of tunnelling—costly works, which were almost prohibitory if the object were to make an inexpensive railway—might frequently be avoided. In considering the question of using light engines on railways of narrow gauge, the excellent system which it was hoped might be seen in operation on the Mont Cenis Railway in 1867, under the supervision of Mr. Brunlees, must not be forgotten; and he trusted that an opportunity might be afforded for a discussion of its merits later in the session.

Since the reading of his Paper on the Festiniog Railway,[2] he had had the pleasure of opening for passenger traffic the Talyllyn

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[1] *Vide* Minutes of Proceedings Inst. C. E., vol. xv., p. 358.

[2] *Ibid.*, vol. xxiv., p. 359.

Railway, 8 miles long, from Towyn to Abergenolwyn, on a gauge of 2 feet 6 inches.

Mr. G. H. Phipps said, it appeared to him that, if the carriages were as wide as had been described, in order to make room for them, there could not be much saving in the cuttings.

Mr. T. E. Harrison remarked, that this question seemed to resolve itself into how to make cheap railways in England; and he thought there were two points, which had been omitted in the discussion, which bore essentially on the cost of railways. In the first place his own experience was, with few exceptions, in all the railways he had made, extending over a period of more than thirty years, that the landowners extorted the utmost farthing they could get for their land, either by means of juries or by arbitration; and if landowners expected railway companies to make railways into their districts, under that system, he was satisfied in future years they would be mistaken. There was one case he could refer to as an honourable exception to the course which was generally pursued. The late Lord Carlisle was very anxious to get a line from Thirsk to Malton. He was a large landowner himself, and had sufficient influence in the district to induce the whole of the landowners over twenty-two miles of line, with only a few exceptions, to agree before the Act was obtained—because unless that was done there could be no confidence in mere general statements —to sell their land for £60 an acre, including severance and tenants' damages. His lordship himself took a large interest in the construction of the line, and he and the other landowners found one-half of the capital. Under those circumstances the whole cost of the line of 22 miles, including stations, was under £100,000, and it now paid about 4 per cent. The other point to be considered in connection with the cost of railways was the requirement which the Board of Trade insisted on—that, with very few exceptions, public roads should not be crossed on a level. He was satisfied the persistence of the Board of Trade in that requirement was, in many instances, quite unnecessary. In some cases a portion of country was traversed where not more than two or three carts, and very rarely indeed a carriage, passed in the day, and it involved a hard contest to get a level crossing sanctioned under any circumstances. In the case of a main line of railway, and an important road, he always advocated a bridge in preference to a level crossing; but he did not think the Board of Trade exercised a just discrimination in their general prohibition of level crossings; the nature of the line, and the objects to be obtained by it, should be more carefully considered. So long as it was the rule to have no level crossings, cheap lines of railways could never be made in this country. He had been accustomed for many years past to study the statistics of the traffic of railways. In a purely agricultural district, without either manufactures or mineral productions, the calculation of what the cost of the line ought to be, to make the traffic pay, was of a very simple kind. He was satisfied that the results of actual traffic to be produced from any line of that nature were not more than from £7 to £10 per mile per week; and he took that traffic return as the basis of what the cost of the line ought to be, which, to pay 5 per cent., and with 50 per cent. for working expenses, ought not to exceed £3,600 and £5,200 per mile, respectively. Generally speaking, in this country a mistake was made at the commencement. Instead of estimating what the country was likely to produce, and then saying, unless the line could be constructed for a certain price it would not pay 5 per cent., lines were laid out without regard to the question whether the traffic was likely to pay or not. He was satisfied, if promoters would take a more commercial view of the matter, and if the lines were constructed only at such a cost as to afford reasonable prospects of the traffic giving a fair return upon the capital, there would be found even at this time, abundance of people ready to invest their money; but when a line was made at a cost commensurate with a traffic of £20 per mile per week, whilst the actual traffic only produced £10, then, he thought, people would be acting an insane part to put their money into it.

Mr. W. B. Adams said, with regard to the general consideration of light and heavy railways, it involved chiefly the question of cost. He agreed that a line should be constructed in reference to the traffic that was to come upon it, paying 10 per cent. on the outlay, with one-half for working expenses; but if it was supposed that railways could not be made for less than £30,000 a mile, then of course it could not be expected that such lines would be provided in poor or sparsely peopled districts. If it was worth while to improve the traffic by a cheaper kind of haulage, substituting the locomotive for the horse, regard must be paid to the work to be done. With reference to the curves, one would hardly venture to say what they might ultimately be brought to. Captain Tyler had given an account of what was done on the Festiniog Railway; but an engine was now being built in Wales to go round curves, not of chains, but of feet, and of so small a radius as 17 feet 6 inches. The gauge was 3 feet, and he supposed that it was thought it would pay. With regard to the saving in width of gauge, it could only have reference to the lightness of the rails, the extent of interval between the sleepers, and the amount of earthwork. Other things being equal, a less cost made the narrow gauge desirable; but whatever the gauge was, the bodies of the carriages to run on it might be made twice the width of the gauge. If it were a question of running the general stock of a main line on light railways, he could not see any particular advantage, and all the saving then obtainable would be in the weight of iron put into the rails, and the diminished number or size of the sleepers. There must be the same strength of railway while the gauge was the same, or the heavy engines would destroy it. Now that the point of putting comparatively light engines on the light rails, with the load distributed over many wheels had been arrived at, the advantage was, the rails were not destroyed by the superincumbent weight; but if the ordinary stock of the main line were used, a large source of mischief would remain. It was an axiom of the late Mr. R. Stephenson, that more damage was done to a railway by bad rolling stock than by heavy engines. With regard to making the engine double, four cylinders in one frame, or two separate frames with two cylinders each, it was a question of convenience. He had no doubt the engine described by Mr. Fox would work well round curves. Whether it was desirable always to have the pressure the same, taking the whole weight of the engine for traction, he was not sure. It was contended that two separate engines would do quite as well in traction, with various other advantages, but if coupled by merely chain-links, a portion of power would be lost in snatches by varying speed. To make two engines work well together, they should be coupled by a universal joint. It was well known that two horses in a carriage rarely developed double the power that each horse exerted separately; and thus the double engine, not subject to this irregularity, would have the advantage. Whether it be doubled by a single frame and boiler with travelling bogies, or by a universal joint, was a question worth considering. In his own case, he generally tried to adapt the tender as a means of increased traction, making it radial to the engine, and by putting friction-wheels between the driving-wheels of the engine and the front wheels of the tender, with a simple arrangement by which the driver could couple and uncouple the wheels as might be required, according as he had a heavy train or a light one to start, or an incline to surmount. A disadvantage in many engines was, that the cylinders were placed in front of the leading-wheels. If they were large cylinders, they acted like a weighted lever on the wheels. He preferred to put the leading-wheels in front of the cylinders, making them radial, and in that case the cylinders, being supported between the driving and the leading-wheels, did not produce any injurious effect. This leverage was very mischievous in two ways, damaging the line by a kind of ploughing action, and taking weight off the hind coupled driving-wheels, to remedy which it was not an uncommon practice to load the foot-plate with a large amount of cast iron as a counterbalance to the cylinders. The double bogie engine had overhanging cylinders, but very small ones, only 11 inches in diameter, and so of comparatively little consequence. A pair of cylinders of 15½ inches in diameter would develop the same power, and with radial wheels in front would not overhang. He had no doubt in some cases the one system was as advisable as the other; and in Mr. Fox's case the engines were intended for special mountain work, and not for general purposes. If he might refer to his own plans, he remarked that Mr. Fox had adopted the spring tires, the advantage of which was, when the engine was going round a curve, the tire slid round the wheel instead of slipping on the rails. One effect of this was to give about 20 per cent. more adhesive value by an elastic fit to the rails, and another, to prevent torsion of the axle; and the tire yielded only just as much as was necessary, in fact, as a friction clutch; and the breaking of a tire under those circumstances would be almost impossible. The reason of tires breaking was generally because they were put on too tight, a system which seemed to have descended from the wheels used on ordinary roads, and when railways were constructed the same system was adopted, and the practice was still continued; but he thought the tire might, with great advantage, be loose, with the certainty that it would never burst. The practice of through bolting the tire also took away one-third of its strength, and on receiving a sudden blow, when it was in a state of greater tension in frosty weather, it broke. His own impression was, that the loose tire would remove a serious element of danger on railways, and would greatly diminish noise, jarring, and destructive wear. If loose wheels were used, they must have a boss, as long, at least, as half the diameter of the wheel; if not, the flange pressure would have a tendency to wear out the bore. Some years ago, happening to be in Mr. Hague's factory, he found there some loose wheels, which had been made for the Great Western Railway. Mr. Adams then asked Mr. Hague how long he thought they would last, when he replied, in his opinion, about a fortnight. The pressure of the flanges of the wheels, 4 feet in diameter, against a short boss of 9 inches, put the wheels to such a severe strain and wear that they had to be taken out again. He tried the principle on the Birmingham and Gloucester line with 3-feet wheels, the boss being about 15 inches. The wheels did very well for about six months, and then began to wear loose. To make loose, or independent wheels, would necessitate a better structure and more cost. The surfaces would require hardening or bushing, and more space would be taken up by the extra size and length of boss; but either the loose wheel or the loose tire must be used, if free action and diminished friction were wanted, as the great object was to get rid of the 'rolling friction.' He preferred the loose tire, either a spring tire, or a hoop tire between the tire and the wheel, as giving all the effect of a loose wheel with greatly increased safety and diminished cost. A large portion of the noise and vibration in a railway train was induced by the skidding of the wheels on the irregular pathway.

Mr. R. Peacock remarked, that the weights of the engine described in the Paper, corresponded very nearly with the leading dimensions of two tank-engines which had recently been built by his firm for the Norwegian Railway on the 3 feet 6 inches gauge. He would briefly give the dimensions of those engines. The cylinders were 11 inches in diameter, with a length of stroke of 18 inches. The leading-wheels were 2 feet in diameter, and the driving and hind wheels 3 feet 9 inches in diameter. It would thus be seen that the tractive power of the engines was not very different; but he found there was a great difference in the dimensions of the boilers, particularly as regarded the heating surfaces, and it struck him either that the Norwegian boilers must be too small or that those of Mr. Fairlie's engine must be too large: nevertheless, the former engines were reported to make steam extremely well. The boiler of the Norwegian engine was 8 feet long, 2 feet 11 inches in diameter, with one hundred and seven tubes of $1\frac{5}{8}$-ths inch diameter; the heating surface was 416 square feet in each of the Norwegian engines, against 1,000 square feet in Mr. Fairlie's engine. And although the Norwegian engines carried only 300 gallons of water, with a fuel capacity of 23 cubic feet each, nevertheless, the weight of each of those engines, in working order, was 16 tons 19 cwt., whilst that of the double engine was given as 30 tons only. He should be glad to know whether the weights given in the Paper were calculated or actual weights, as it seemed to him there must be some mistake. As to whether the double engine or engines constructed on the ordinary principle were preferable, it must be allowed that two independent engines, arranged foot-plate to foot-plate, could be made a better mechanical job than an engine built with loose independent frames, and attached to the boiler by centre pins. He contended that the cost of working two engines placed foot-plate to foot-plate would not be more than the cost of working the engine described in the Paper, inasmuch as one driver and fireman could attend to both. Another feature in favour of two independent engines was, that in the case of the break-down of one engine, it could be detached, and a fellow-engine could be put to the one remaining; and thus only half the engine-power would be laid up for repair, as against the whole engine-power in the other case. He did not consider the engine described in the Paper possessed anything like the simplicity or stability, as a mechanical structure, that two separate engines did; neither did he think it could be kept up at the same cost. As a locomotive manufacturer, he would sooner build two independent engines of equal tractive power, for the same money, than one similar to the engine described in the Paper, and he thought the two engines would be found much less costly in repairs. The weight on the leading wheels of the Norwegian engines was 4 tons 2 cwt., on the driving-wheels 6 tons 12 cwt., and on the hind wheels 6 tons 5 cwt.; but as the engines were provided with equal compensating levers between the driving and hind wheels, the weights upon these wheels would be found equal, after the engines had run a short time.

Mr. Fox, in reply upon the discussion, said it was only proposed to use Mr. Fairlie's engine on those parts of the line which had very sharp curves and severe gradients; and the reason why, after a great deal of consideration, it was decided to adopt it, instead of two engines foot-plate to foot-plate, was, that by using two engines suitable for sharp curves, the advantage of the adhesion of the weight on all the wheels could not be obtained. By Mr. Fairlie's principle, they got 3 tons on each wheel, whereas with two engines, there were two wheels of the six which had no effect upon the tractive power. In the Queensland engine there was no waste weight, all being available for the purpose of haulage. A similar engine had been at work for some months, with eight wheels instead of twelve, and on this he had made a most satisfactory trip. With reference to the inquiry as to the quantity of earthwork per mile on the Queensland line, he would state that of the total cost of that part which came out at £9,000 per mile, the proportion for excavation was about £2,500. On the part costing £6,000, the excavation was £1,050. He could not state what was the price per yard, but it was probably about 1*s.* 6*d.* for ordinary soil. With reference to what had fallen from Mr. Harrison, he would say that the question of the calculation of cost proportionate to the traffic of the line was broached by Mr. FitzGibbon directly the Government decided upon making railways in Queensland, and they began by looking at the traffic which actually existed at the time. The Appendix to the Paper, giving the calculations of Mr. FitzGibbon, the Chief Engineer, showed what the minimum traffic would be, and it was then estimated what traffic might be expected, based upon the old calculations of the first railways in England. It was estimated that the minimum traffic on the Queensland line would pay 8¼ per cent. on the capital expended, and letters from the colony stated that 8 per cent. had been realized. It seemed there was an apprehension, that he advocated break of gauge, and the introduction of the narrower gauge in England. He did no such thing, though this might, in certain cases, be desirable. His object was to lay down an important principle, the reduction of the weight on the driving-wheels, by using an increased number of wheels, and by making the rolling-loads as light as possible. He believed, with due care, railways might be made in England, through an ordinary country, for £5,000 per mile. As to the sharp curves and steep gradients, they were only used on particular parts of the line—over mountain ranges—therefore much of the saving in an ordinary country could not be attributed to sharp curves. If they had not been adopted on the Main Range, the expenditure would have been ruinous; and if the minimum radius had been 8 chains instead of 5 chains, the cost of that portion of the line would have been at least £30,000 per mile more. But that was not the general reason why these light lines had been cheaply made. It was because all the parts were properly proportioned, and made as light as they could be. In earlier days, when labour was cheap, it would no doubt have been better to use the excellent native timber, than to have sent out iron structures from this country ready to be fitted up. With reference to the loose wheel, he had only tried it as an experiment. Mr. Adams had stated, that the bearing ought to be one-half the diameter of the wheel, and that was just what it was in this case. Railways should be proportioned to their traffic, and it was important to have as light a weight on the driving-wheels as possible; as by so doing the weight of every part of the structure might be kept down.

December 4, 1866.

CHARLES HUTTON GREGORY, Vice-President,
in the Chair.

The following Candidates were balloted for and duly elected:—Peter Ashcroft, John Chester Craven, William Johnstone, Thomas Martin, A.B., James Raine Rushton, and Arthur Anderson West, as Members; James Abernethy, Thornton Andrews, John Charles Ardagh, Lieut., R.E., Robert Dudley Baxter, James Blackburn, William Wingfield Bonnin, Thomas Brassey, Jun., Albert James Leppoc Cappel, George James Crosbie Dawson, George Eedes Eachus, Joseph Breedon Fryer, Thomas William Gardner, Lewis Conway Gordon, Lieut., R.E., John Thewlis Johnson, William Mercer, Charles James More, Charles Mumford, Francis George Shirecliffe Parker, Capt. 54th Regiment, Michael Patterson, William Ridley, Christer Peter Sandberg, William Brounger Taylor, James Farnham Tuson, and Edward Henry Woods, as Associates.

The discussion upon the Paper, No. 1,166, on "Light Railways," occupied the whole of the evening, to the exclusion of any other subject.

December 11, 1866.

CHARLES HUTTON GREGORY, Vice-President,
in the Chair.

No. 1,134.—"On the best Means of Communicating between the Passengers, Guards, and Drivers of Trains in Motion."[1] By William Henry Preece, Assoc. Inst. C.E.

The establishment of some means of communication between the passengers, guards, and driver of a train in motion is a question which has occupied the public mind, with varying degrees of intensity, at different periods, since the introduction of railways. In the early days of railway travelling, every carriage was accompanied by its guard, and there existed, therefore, no necessity for any mechanical contrivance to attract his attention. But when he was transferred to a protected compartment, and the trains were increased in size and number, the convenience of any means of communication was overlooked.

The railway system has attained its present position and proportions by a series of compromises, and perhaps one of the most striking features of its growth is the peculiar gradation in which the former mail coach has been transformed into the present isolated state saloon. If an entirely new system of locomotion had now to be commenced, circulation and communication in trains would perhaps be one of the first points considered; but inasmuch as experience has grown with the traffic, and the traffic has far exceeded the most sanguine expectations, the arrangements have acquired such complication, that the adoption of a safe and reliable system of communication is now a most difficult problem.

The question has at different times been fully examined and discussed. The Railway Commissioners, and subsequently the Board of Trade, frequently urged it upon the attention of the railway companies. It has on two occasions formed the subject of inquiry by Committees of the House of Commons; and has three times been referred to a Committee of General Managers by the Railway Clearing House.

The Committee of the House of Commons, presided over by Mr. Bentinck, M.P., in 1858, presented a report, in which it was recommended that it should be imperative on every railway company to establish a means of communication in trains. Mr. Bentinck has annually, since that period, asked the Government whether it was intended to introduce any measure founded upon the recommendations of that report; but the Government has invariably refused to assume the responsibility of taking the management of the lines out of the hands of the railway companies. Mr. W. B. Sheridan, M.P., during the recent session, obtained leave to bring in a bill to render it compulsory upon railway companies to establish a communication between passengers and guards. The bill was referred to a special committee, who reported that it was impossible to legislate upon the subject during that session, but recommended that it should be brought forward again on the earliest occasion.

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[1] The discussion upon this Paper extended over portions of two evenings, but an abstract of the whole is given consecutively.

The Committee of General Managers, appointed by the Railway Clearing House, in February, 1852, to consider the propriety of establishing a passage for the guard from end to end of the train, reported against that proposal. In the same year another Committee was appointed, and after careful investigation, a report was issued, in March, 1853, opposing the introduction of any passenger signals, but recommending the adoption of a simple cord and bell, to enable the guard to attract the attention of the driver. In the latter part of 1864, the Board of Trade called the attention of the Railway Clearing House to the matter, and a Sub-Committee of General Managers[1] was again formed to investigate and report upon the subject. The public was invited to send in designs, and more than two hundred were submitted. These were carefully examined, and several were selected for trial. A report was issued in April, 1865, and the conclusion arrived at was substantially the same as that of the previous Committee; but it was admitted that some contrivance was advisable, in express trains, to enable passengers to attract the attention of the guard, and it was recommended that experiments should be continued, with the view of ascertaining the best means of effecting that object. At the same time the Board of Trade directed Captain Tyler (Assoc. Inst. C.E.), one of their inspecting officers, to investigate and report upon the subject. This report, which is exhaustive and complete, was issued in April, 1865, and was in favour of the establishment of some means of communication in trains.

The subject has received equal attention in France. In 1861, the French Government appointed a commission of inquiry into the management of railways, and especially as to the question of passenger signals. In a report published in 1863, it is stated that the French railway companies, like those of England, unanimously oppose the introduction of any passenger communication, but approve of the signal between the guard and the driver.

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[1] Including Messrs. Forbes (L. C. & D.), Allport (Midland), Cawkwell (L. & N. W.), Seymour Clarke, (G. N.), Eborall (S. E.), Grierson (G. W.), Hawkins (L. B. & S. C.), Moseley (G. E.), and Archibald Scott (L. & S. W.).

Such is a history of this matter up to the present time. The Sub-Committee of the Railway Clearing House has again been formed, and is now occupied in investigating the results of the experiments that have been made since the publication of their report. The measure, as regards its general adoption, is therefore still in abeyance.

A careful consideration of the nature of the accidents that occur on railways, and of the risk to which the lives of passengers are exposed, through the interval of time that so frequently elapses before the driver becomes aware of the presence of danger, must satisfy the most sceptical that, if there be no absolute necessity for the free communication between passengers, guards, and driver, some means of attracting the instant attention of the servants of the company who have the control of the train is imperative. The limited mail and fast express trains frequently run eighty miles without stopping, and the tendency is even to extend this distance. Under these circumstances, passengers may be confined, in helpless isolation, for two hours or more; and cases have occurred where a disabled carriage has been dragged twenty or thirty miles, without the passengers having the power to escape, or to attract the attention even of the guards in the rear of the train. This is admitted by the railway companies; but the objection that has hitherto prevented the introduction of some general system is the unwillingness of the railway authorities to place the control of the train in the hands of the passengers. Delays are sufficient already, without giving the passengers additional means of creating them, and at the same time increasing the risks of railway travelling. These objections, as regards the danger of stopping trains, are to a great extent removed by the introduction of the "block system" of working upon railways, whereby two following trains are not allowed to run upon the same section of line at the same time; but at present it is rarely adopted. That these objections are exaggerated, and can be removed, is proved by the experience on the London and South Western line, where several trains have been running uninterruptedly for nearly two years, fitted up with passenger signals, and where a needless alarm has only once been raised, and then the delinquent was instantly detected.

Before describing the mechanical construction and working of the system adopted on the London and South Western line, it may be well to glance, briefly, at what has hitherto been attempted and proposed. The efforts of those who have directed their attention to this matter, have been not merely to afford communication between passengers and guards, but some means of access for the guard to the passengers. It has been proposed to establish a foot-board and handrail upon every carriage, to enable the guard to patrol the train while in motion; but the construction of the platforms, the breadth of the permanent way, and the width of the rolling stock, preclude the possibility of carrying out this contrivance, without endangering the lives of the servants who had to traverse the train. Moreover, such a contrivance would facilitate robberies and assaults, and would expose the public to greater dangers than already exist. The carriages on several Irish lines are fitted up with foot-boards; but beyond being of occasional use to enable the guard to shut a door whilst the train has been running, they have not been employed for general circulation. The system is much used on the Continent, where there are facilities for its employment. In Belgium, it is calculated that one guard per annum loses his life in traversing trains. In England, the deaths would average ten per annum, mile for mile; but would be much greater taking train for train, were it possible to carry out the same system.

As external circulation is, therefore, neither practicable nor desirable, Captain Tyler advocates internal circulation, upon a modified arrangement of the American plan. A system of this description has been partially adopted in Switzerland. But the Sub-Committee of General Managers reported, in 1852, that—

"23. And as regards the American arrangement, it is obvious that it is so much opposed to the social habits of the English, and would interfere so much with the privacy and comfort which they now enjoy, that these considerations, apart from others nearly as important, would forbid its adoption in this country."

And again, in 1865, that—

"11. As to the suggestion, that the English companies should adopt carriages built upon the American plan, which allows of internal communication from one part of the train to the other, by means of a central passage, the Sub-Committee is satisfied that the habits of English travellers would not tolerate any such system. Apart from the enormous outlay which would be involved in a reconstruction of the stock of the different companies to secure a doubtful advantage, the delays which arise in the loading and unloading of trains on the great railways of England, particularly in the metropolis, and at other great centres of population, would be productive of very serious evils. Any person who has travelled upon railways where these carriages are in use, must have observed the great loss of time which is consequent upon the means of leaving and entering them, being limited to the door at each end, instead of taking place by the more numerous doorways parallel to the platforms, as in the ordinary English carriages."

Both the French Commission and the Committee of the Railway

Clearing House urge, that communication without circulation is comparatively useless; but, as Captain Tyler has remarked,[1] "a means of communication is even more required when there is no opportunity for circulation, than when the power of circulation exists. Without either communication or circulation, the passenger is comparatively isolated and doubly helpless. Free circulation gives of itself an admirable means of communication. Ready communication makes up to some extent for the want of circulation."

The French Commission of 1861 reported favourably upon the system of fixing glass partitions between the compartments of carriages, and the plan has been successfully tried in England. The carriages on the Cork, Blackrock and Passage Railway, are fitted with plate-glass windows in each partition between the compartments. The London and South Western Railway have fixed in all their recent passenger coaches, a revolving circular glass window, about 15 inches in diameter, centrally in the partitions between the compartments, and 3 inches below the roof. It has met with much approbation; but while it affords a means of partial publicity, and therefore security, it destroys to a great extent the privacy of the present system, and affords no remedy for the isolation of the carriage. Moreover, its employment, to be at all beneficial, necessitates the occupancy of every compartment of every carriage; and while it may afford some protection against outrage, it would not, in cases of accident, supply the means of attracting the attention of the guard.

Failing the adoption of any system of internal or external circulation, what is really required is, that the instant attention of the guards and driver should be directed to anything of a serious nature occurring to the train whilst in motion, requiring their immediate assistance. Travelling porters, maintaining a careful watch over the train, from a box placed on the tender, were employed for many years on the Great Western line. The vans of the London and North Western, and other lines, were so constructed as to allow the guard to take a similar survey; but it is evident that such systems are useless at night, in tunnels, or during foggy weather. Moreover, as the guard is employed in sorting luggage, parcels, &c., it requires a special servant to accompany every train, to render this plan at all effective, and then only during daylight. Mirrors have been placed in front of the driver and guard, in which the train was reflected, but they have many defects, the principal being the almost impossibility of keeping them sufficiently clear and dry to reflect. These mirrors have been found occasionally useful, in enabling the driver, when leaving a station, to see any signal that may be given, without requiring him to look round.

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[1] *Vide* "Quarterly Journal of Science," vol. ii.

A system of communication to be at all effective must be placed within the control or reach of the passengers themselves; and numerous plans have been devised to accomplish this object. The simplest would appear to be, the extension into each passenger carriage of the ordinary cord and bell, or cord and whistle, which is extensively used in this country and on the Continent between the guards and driver. Many railway companies start and stop their trains in transit, and during shunting operations, by means of this communication, but it has frequently failed in action, even in its present simple form, in long trains, and the system is not adapted for further complication. The liability of the cord to become entangled at the fastenings, to jamb over the pulleys and guides, the accumulation of friction in long trains, the variations in length from the extension and contraction of the train, are all opposed to its success in practice. An attempt has been made to use the cord and bell for passenger signals on the Great Northern Railway, but after several months' trial it failed, and has been abandoned. The Midland Railway Company experimented upon a plan of this nature, but the results were of such an unsatisfactory character as to convince them of its inutility. The same objections apply, but to a much greater extent, to any mechanical contrivance extending the whole length of the train; for there are not only the difficulties of friction, expansion, and contraction, but the extreme variations in dimensions and form of the various rolling stock of different railway companies. It has, therefore, been attempted to isolate the alarm to each separate coach, and thus to avoid the difficulties of connections and couplings required in the making up of a train.

Experiments have been made, but without success, to economise the mechanical energy of the train in motion, by compressing air in suitable cylinders, to sound whistles, trumpets, and bells. The violent current of air generated by the passage of the train through the atmosphere has been utilized to sound a bell upon the roof of each coach. Immense gongs, fireworks, detonating signals, powerful means of generating light and exhibiting signals, have also been suggested. But the Committee of General Managers reported in 1853, that experiments demonstrated that not even "the loudest steam whistle could, if placed in the rear of the train, be depended on as a signal to the engine-driver." "Signals dependent on sound are as little to be relied on as those depending on sight."

The French Commission report, "that experiments prove that visual signals cannot be seen, and aural signals cannot be heard;" and the conclusion they arrived at was, "that electrical communication was the only solution of the problem." But the Committee of the Railway Clearing House of 1852 reported that, "the electric fluid is too subtle, and the apparatus used for evolving and conducting it too delicate, for the rough usage and the disturbing causes which they would be exposed to when trains are in motion."

The Author would observe that, however sound this argument may have been in 1852, it will not hold good in the present day. There is no force so ductile, so pliable, so simply generated, so easily controlled, and so readily manipulated, as electricity. It is obstructed by no friction, like mechanical motion. It requires no powerful appliances to restrain its action, like steam. It demands no tubes, pipes, valves, cranks or other mechanical agencies to guide it, like water, gas, or air. All that is wanted is a plain wire, that may be bent or laid in any direction, needing only the simplest contrivance to retain it in its course. The laws and the action of electricity are thoroughly understood. There is no tension, and consequently, no breakage. There is no motion, and therefore no sticking of parts. The necessity for delicate machinery, which the supposed subtlety of the force demanded, no longer exists. Experience shows, that the stouter and more durable, within certain limits, the mechanical construction of electrical apparatus, the better it works. Indeed, the oscillation of a railway train is eminently adapted to maintain those surfaces bright and clean upon which the passage of the electric force materially depends. It has been tried upon the London and South Western, the Midland, the Great Northern, and the South Eastern Railways, and has proved successful.

The use of electricity for communicating in trains has often been attempted, one of the earliest experiments having been made twenty years ago upon the London and South Western line. In 1855 the London and North Western Railway Company spent a considerable sum of money in endeavouring to establish a system of electrical communication between guards and driver, but it proved unsuccessful, principally from the delicacy of the apparatus, the failure of the couplings, the insufficient insulation of the wires, and the absence of fish joints to the rails. In France, the "Chemin de Fer du Nord" have been experimenting for years, and have spent large sums in endeavouring to perfect an electrical arrangement. After full experience of its practicability, the whole of the rolling stock, comprising twelve hundred carriages and trucks, has been fitted with an electrical communication between the guards and drivers. The Lyons Railway are now following this example, and the result of its working has been so satisfactory, that the French Government has recently called upon all the railway companies in France to adopt some such system without delay.

The general arrangement of the system introduced by the Author is shown in Plate 7, Fig. 1. One wire (electrically) traverses the whole length of the train, the couplings between the vehicles being formed of stranded insulated wire, as shown in Fig. 4. In order to avoid any difficulty in reversing the carriages, each end is fitted with a hook and eye (Fig. 5), placed so that in whatever direction the carriages run, the couplings are always relatively together. The insertion of two roads for the passage of the electric current enhances considerably the security of the coupling, because if one point of contact fail, there is a second to maintain the communication. Every engine and van is fitted with a bell and battery, the similar poles of which are connected through the framework and wheels with the rails, while the opposite poles are attached to the train wire, so that whenever the wire traversing the train is brought in connection with the rails, through the framework, &c., the bells in each van will sound, through the disturbance of the electric equilibrium of the wire.[1] The ordinary screw couplings are also connected together, so that if the rails fail to transmit the current, through the absence of fished joints, another course is open for its return to the battery. An ordinary trembling bell (Fig. 6), but of increased dimensions, is used, which will give any required code of signals, or will continue to ring incessantly, as long as the current is maintained. It is provided with a locking crank, which prevents any sound from being emitted by the oscillation of the train, and which allows the bell to ring only when a current flows. Each guard is able, by means of an ordinary commutator, to communicate with the driver, and with every van. A code of signals has been drawn up, to enable the guard to forward proper instructions to the driver (Appendix No. I., page 90.) It has been found in practice, that bells, however loud, cannot be depended upon to attract the attention of the driver. He has therefore been provided with a signal (Fig. 7) upon the weather-board, which exhibits a red disc and the words "Look out" by day, and a red light by night. Every compartment has also the means of ringing the bells in the guards' vans, and if necessary, on the engine, produced by the same agency as that at the command of the guard, but protected from the mischievous, idle, and timid, by glass, which has been found to be the best material for the purpose. An external as well as an internal signal upon each carriage was originally used, but recently the external apparatus has been abandoned.

The apparatus placed in the carriage, which is operated upon by the passenger, is shown in Fig. 3. It is called the 'releaser.' A is an automatic arrangement, upon the principle of a 'Jack-in-the-Box,' which, on the fracture of the glass face, at once lowers the semaphore arms outside the carriage, and raises the alarm. The releaser consists of a round mahogany box, having a hinged cover, which is locked with a spring lock. At each end of the box there is a glass face, one being in each compartment. Inside the box is placed a brass cylinder, having a piston-rod working through it at both ends. These rods are acted upon by an internal spiral steel spring, the action of which is to thrust them outwards. Attached to cross-heads upon these rods are fixed knee-joints of steel, which on their extension, draw wires that lift cranked-shaped catches, thus releasing the semaphore arms, and allowing them to fall by their own gravity. Now the effect of the glass face of the locked cover is to restrain the action of the spring upon the piston-rod, and to maintain each part *in statu quo*. The moment, however, the glass face is destroyed, the piston-rod flies out, the detent is released, and the semaphore arms fall, making contact between the wire and earth, ringing the bells, and attracting the attention of the guards and driver. The piston cannot be thrust back, the semaphore arms cannot be restored to their normal position, the bell will not cease to ring, until the broken glass face is renewed by the guard. In some cases this 'Jack-in-the-Box' arrangement was dispensed with, and a ring inserted, which, on being pulled by the passenger, released the detents and allowed the semaphores to drop. When the external signals were dispensed with, the apparatus shown in Fig. 7 was replaced by a simple commutator, Fig. 8, which, on being moved by the passenger in the direction of the arrow, placed the train wire in contact with the earth, and raised the alarm. The movement of the handle locked it in a position which could only be replaced by the guard.

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[1] The principle of electric equilibrium is subsequently described, and is illustrated in Fig. 2.—W. H. P.

It is believed that such a contrivance, with or without the external signal, entailing the fracture of a glass face, would only be sought and used by passengers in cases of great excitement or danger; and that the adoption of such an apparatus destroys the only tangible objection raised by the railway companies to the introduction of a means of communication between the passengers and guards. This opinion has been borne out in practice. The experience gained from nearly two years' working upon the London and South Western Railway is, that whatever difficulty exists in establishing a system of communication between passenger and guards, is merely a mechanical one; and that any danger or inconvenience from placing such a convenience in the hands of the passenger, is purely chimerical. It must be observed, that the control of the train is in no case withdrawn from the servants of the company. The passenger simply attracts the attention of the guard, who exercises his own discretion whether to stop the train at once, or to wait until it arrives under the protection of the next fixed signals. It is, however, a question for discussion, whether railway companies should not be additionally protected, by legislative action, from the depredations of the foolish and the evil-disposed. The infliction of a penalty for any unnecessary stoppage to the train would unquestionably prevent tampering with the communication.

The system also contains a contrivance by which instant intimation is automatically conveyed, to both guards and driver, of any broken couplings, or accidental rupture of the train. When the eye (Fig. 4), is forcibly drawn away from the hook, the helical spring contained in the barrel (B) forces the hook against the metal stud (E), and at once raises the alarm. Accidents are so sudden and violent in their nature, that a passenger may be rendered perfectly powerless to raise an alarm himself. The value of an automatic arrangement for this purpose is evident. One great advantage of this self-acting system is, that until every carriage is properly coupled up, the bell will ring, and the attention of the guard be forced to the proper making up of the train. If the bell do not ring, the attention of the guard is called to some irregularity in the communication, so that either way the result is beneficial, and the necessity exists of constantly maintaining the system in correct working order.[1]

Such, generally, is the system which is now in practical use. Considerable modifications have, from time to time, been made in the details, to suit the requirements of different companies; and while the modifications adopted on the London and South Western line meet the demands of that Company, the arrangements are such, that although every line may have its peculiarities of traffic needing special attention, this system, as a system, is universally applicable to every train, as it allows 'foreign' carriages to be introduced, or even wagons and trucks to be inserted, without interfering with the communication. It is believed that it fully solves the question of communication in trains in motion, and it has at least proved to be successful in practice.

The communication is accompanied by a series of Diagrams, from which Plate 7 has been compiled; and is further illustrated by specimens of the various instruments.

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[1] This self-acting arrangement has been abandoned, from its delicacy and unfitness for rough work.—W. H. P.

APPENDICES.

No. 1.

THE following is a copy of the Instructions issued to the Guards and Drivers of the London and South Western Railway :—

SOUTH WESTERN RAILWAY.

INSTRUCTIONS TO ENGINEMEN, GUARDS, AND ALL PARTIES CONCERNED.

Electric Communication between Passengers and Guard.

Some of the Trains are now fitted up with an Electric Communication between Passengers and Guards. In the event of something of a *serious nature* occurring, which URGENTLY requires the stoppage of the Train, the Passenger may "Break the Glass," and "Ring" by moving the Bell handle in the direction denoted by the Arrow :—thereby a Bell will ring in each Guard's Van in the Train, and also on the Engine.

When the Guards and Engineman hear the Bell ring, they will at once *look carefully along each side of the Train*, and in case any violent oscillation be seen, or a carriage be on fire, or other occurrence of a serious character be observed, *the Train will be stopped* as speedily as possible, and, when stopped, must be protected by signals, as prescribed by the Rule Book.

Should, however, the Guards and Engineman fail to observe anything *which really necessitates an immediate stoppage* of the Train, their duty will be to stop the Train at the next Station or Junction, so as to protect the Train, when stopped, by fixed signals.

When the Train is stopped the Passenger who broke the glass and rang the Bell will communicate with the Guard; but should he fail to do so, the Guard will detect the compartment from which the Passenger gave the alarm, by looking for the broken glass, and in case the alarm has been mischievously and wantonly given, or from insufficient cause, the names and addresses of all the Passengers in that compartment will be taken, in order that the law may be enforced.

THE BELL SIGNAL CODE

Between Guards and Engineman is as follows :—

One Beat	Acknowledgment.
Two Beats	Go on ; all right.
Three Beats ...	Look out ; something wrong.
Four Beats	Shut off steam ; pull up, and stop at next Station or Junction.
Six Beats	Danger ; stop at once.

Every Signal must be acknowledged.
The acknowledgment by Enginemen will be given by One Whistle.

BY ORDER.

London, 10*th August*, 1865.

Fig 9.

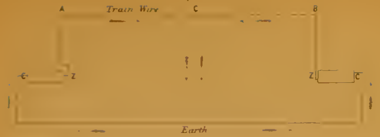


Fig 10.

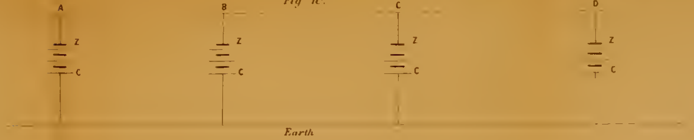


Fig 1.

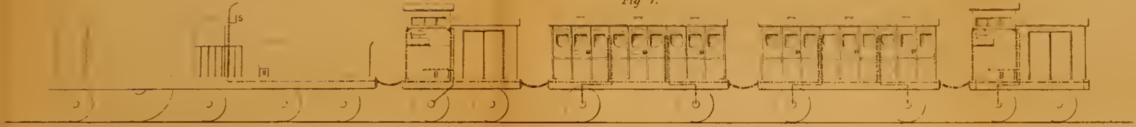


Fig 2.

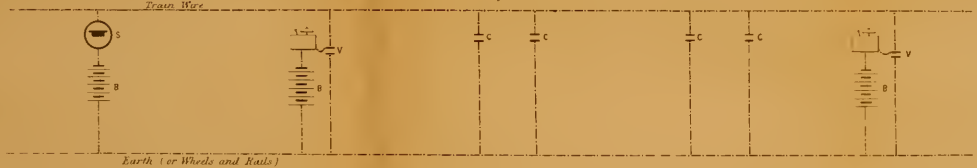
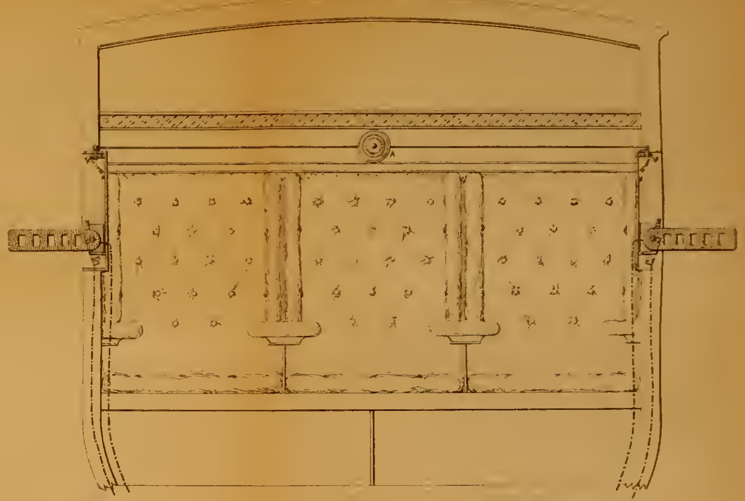


Fig 3.



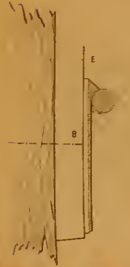


Fig 5.

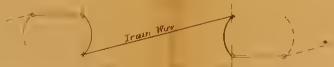


Fig 4.

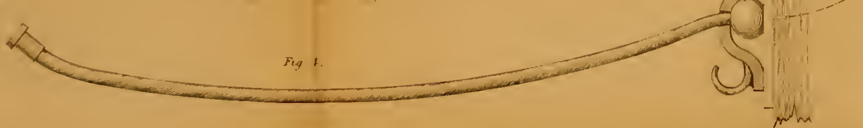


Fig 6.

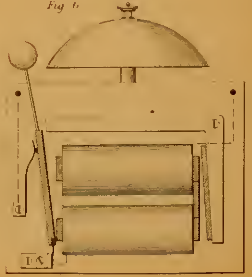


Fig 7.

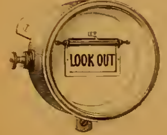
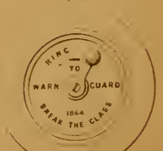


Fig 8.



No. 2.

Instructions inserted in Carriages:—

SOUTH WESTERN RAILWAY.

Electric Communication between Passengers and Guard.

This Train is fitted up with an Electric Communication between Passengers and Guards. In the event of something of a *serious nature* occurring, which URGENTLY requires the stoppage of the Train, the Passenger may "Break the Glass," and "Ring" by moving the Bell handle in the direction denoted by the Arrow:—thereby a Bell will ring in each Guard's Van in the Train, and also on the Engine.

When the Guards and Engineman hear the Bell ring, they will at once *look carefully along each side of the Train*, and in case any violent oscillation be seen, or a carriage be on fire, or other occurrence of a serious character be observed, *the Train will be stopped* as speedily as possible, and, when stopped, must be protected by signals, as prescribed by the Rule Book.

Should, however, the Guards and Engineman fail to observe anything *which really necessitates an immediate stoppage* of the Train, their duty will be to stop the Train at the next Station or Junction, so as to protect the Train, when stopped, by the fixed signals.

When the Train is stopped the Passenger who broke the glass and rang the Bell will communicate with the Guard; but should he fail to do so, the Guard will detect the compartment from which the Passenger gave the alarm, by looking for the broken glass, and in case the alarm has been mischievously and wantonly given, or from insufficient cause, the names and addresses of all the Passengers in that compartment will be taken, in order that the law may be enforced.

Passengers are earnestly requested themselves to protect the Communication from improper and mischievous use, as it is very important that it should not be used without real and urgent necessity.

Mr. Preece thought, after what had been said in the Paper, that it was scarcely necessary to allude again to the advantages of employing electricity as a means of intercommunication in trains. He probably looked upon electricity with a prejudiced eye; but he regarded it as only yet in its infancy, and he believed it would be used for railway purposes to a far greater extent than at present. His impression was (and it was one which had been almost unanimously arrived at by those who had investigated the subject in France and in England) that if the question of intercommunication in trains was to be solved at all, it would be by means of electricity. It was an old subject, and there were several methods by which it could be applied. The conditions laid down for a perfect system of communication in trains were these:—first, there must be a means of communication between the guard and the driver; secondly, between one guard and another; and, thirdly, between passengers and guards. Those were the three great questions left to electricians to solve, and which had not as yet been accomplished by mechanical means. The difficulties had been increased from the circumstance, that all this must be effected with one wire. Taking a train as composed of an engine, two vans—one at the front and one at the rear—and any number of carriages, and supposing a wire to be extended along the train from end to end, it was required that the passengers in any portion of the train should be able to ring the bells on the engine and in the vans. There were several ways of effecting this. The first was by means of what was known as the open circuit, which was the ordinary method by which telegraphs were worked. A battery was inserted in any portion of the circuit, the current flowed through it and set all the bells on the wire ringing. To effect this every carriage, van, and engine, would have to carry its own independent battery. That was, however, an inadmissible condition; and was, therefore, condemnatory of the use of the open circuit in trains. The next means of solving the question was the closed circuit. By this plan a battery, either on the engine or the leading van, maintained a constant current through the wire, which would be stopped by the rupture of the wire at any point, and thereby the machinery would be set in action; but there was this difficulty, that if the current were interrupted by necessity or accident, or even in detaching a carriage from the train at a junction or station, all the bells would ring and a false alarm would be raised. The open and the close circuits were therefore equally objectionable. The next method suggested was that which was usually called the balanced current system. It was this:—if a wire (Fig. 9) extended along the train as before, and a battery were placed at one end, one pole being attached to the train wire and the other to the earth or a second wire, and if a battery were similarly placed at the other end B, with its similar poles attached to the train wire, then there would be a tendency on the part of those two batteries to send currents in opposite directions, and if those two currents were equal in tension, an equilibrium would be established in that wire, and no current would pass through it; but if the wire were connected to the earth wire at C, then the currents would flow in the direction shown by the arrows, and the bells placed at A and B would be set ringing. There was this objection to the balanced current system, that there must be a battery at each end of the train, and those two batteries must be equal in tension, and must be kept opposed to each other with similar poles, and if any reversal of the vans took place, or an extra van was inserted during the progress of the train, the whole arrangement was put out; unless, indeed, certain switches or commutators were provided to compensate for the disturbance. The use of switches was objectionable, because they were liable to be neglected or put out of order by the guards; and if there happened to be one or more vans in the middle of the train, the two end batteries were overworked in a long journey, while the middle battery expended very little force, and the balance was therefore overthrown. The system he had introduced was a novel application of electricity to this purpose. He called it electric equilibrium. It entirely avoided the use of switches, and threw upon each battery an equal amount of work. If a wire or conductor A (Fig. 10) was taken, and one pole of a battery Z applied to it, the other pole being attached to the earth, the wire acquired the tension of that pole. In the same way, if A were a pipe or hollow tube with closed ends, placed in connection with the steam chest of a boiler, it would be filled with steam of the same pressure. Again, if the similar pole of another equal battery was applied under the same conditions to another conductor B, this wire would also be charged to the same tension. Similarly with C and D, and indeed with any number of conductors, all would be charged to the same tension if the batteries were equal; and, therefore, if they were all connected together to form one conductor like the train wire, Fig. 2, no variation would occur; they would all be equally charged, the wire would be in a state of equilibrium or equipotential, and any disturbance of this equilibrium would result in the flow of currents. If, now, at any spot C, say a passenger carriage, this equilibrium were destroyed, by placing the train-wire in connection with that leading to the earth or return wire, a current would circulate which would be maintained by currents flowing from each battery that would set its own bell ringing, or the indicator on the engine moving. Thus a train might be composed of any number of vans and carriages, placed in any position, and yet the bells could be rung from any spot without the necessity of compensating for any change or renewal. There had hitherto been a difficulty in meeting the wants of a train which was broken up into sections in its transit and brought together again on its return, and he believed the only remedy was that of electric equilibrium. In place of employing a second wire to allow the current to return to its source, use was made of the metals on the line, which were placed in connection with the commutator. Every van—not every carriage—contained its battery and bell.

With reference to the breaking of the glass face in the instrument, by means of which the attention of the guard was called by the passengers, Mr. Preece showed that a very slight blow was sufficient to break the glass and give access to the ring or handle, which had to be pulled to give the signal. External signals were of use only in cases of false alarm, and then only in the day-time. For economy, as well as for other reasons, he had abandoned the use of external signals, and confined himself to the means of raising the alarm within the carriages themselves.

A most important feature of his system was the arrangement for communication between the guards. He might mention that this apparatus had been applied on the South Western, the Midland, and the Great Northern Railways. It had travelled over 500,000 miles, but during the whole of that time there had never been occasion to attract the attention of the guards. He hoped eventually to substitute for the 'look out' signal the blowing of the steam-whistle.

Mr. J. J. ALLPORT remarked, that he had been associated with most of the leading railway managers of the kingdom, on every Committee appointed by the Railway Companies to investigate this subject, in 1852, 1853, 1864, and 1865, and the experiments were still being carried on. He was not at present in a position to give a decided opinion, either upon Mr. Preece's, or any other system. He sat on the Committees in 1864 and 1865, when upwards of three hundred plans were brought forward by different inventors, and, he was sorry to say, with very little practical result. He was inclined to adhere to the opinion, which had been frequently expressed, about the extreme delicacy and want of uniform action of electricity for the purposes sought to be obtained. On the Midland Railway two trains had been fitted with Mr. Preece's apparatus, and occasionally it got out of order. On one occasion the whole of the bells were set ringing without apparent cause, and could not be stopped. The apparatus had been at work since last April till recently, but was never of any practical use to the passengers, and it was now discontinued. It was stated in the Paper that the rope-and-bell system had been adopted and abandoned on the Midland Railway. That was not the case.

Mr. Preece said, he only alluded to the experiment of the rope and bell between passengers and guard; it was still continued between guard and driver.

Mr. Allport went on to remark, that the rope and bell was early adopted on the Midland, London and North Western, and other lines. It was still in operation in almost every Midland train, and had been so for the last ten or a dozen years, but in no single instance, out of the hundreds of thousands of trains run in that period, could he recollect that it had been found of any practical benefit. It so happened that the week before last a carriage sheet caught fire between Bedford and Hitchin. On that occasion the ordinary passenger engine became disabled at Wellingborough, and a goods engine, which was there at the time, was attached to the train; but there were no means of attaching the rope and bell to the goods engine. On the sheet taking fire the guard attracted the attention of the driver by putting on the break, and the train was stopped within the distance of half a mile. He mentioned this to show how easy it was for the guard to call the attention of the driver. He was aware that a strong feeling existed on the part of the public to have communication between passengers and guards. It had engrossed the attention of managers for several years, but he was not prepared to say they had arrived at a solution of the problem. The experiments would be continued, and if any satisfactory invention could be applied, if it were simply to satisfy the public mind, railway companies would adopt it at once. But anything which required delicate manipulation, like electricity, and was so seldom required, would, he feared, be found out of order, when wanted. Like everything else out of use, it was generally useless when required. For instance, at many private houses and establishments where fire-engines were kept, possibly unused for years, a fire took place, when it was found either that the hose was rotten, or the valves were out of order, or the engine could not work, and so the premises were burnt down. So it would probably be with this electrical machinery, when required it would be "found wanting." But at the same time, if some simple and efficient means could be devised, railway companies would be glad to adopt it. He could understand that on the London and South Western it had worked well for two years. Mr. Preece himself was connected with it. If it were properly attended to, and watched after the running of every train, and all the parts looked to, the apparatus might remain in order; but in the routine of the ordinary staff of a railway, these matters were not generally attended to, more especially when for periods of three or four years together, it was never required to be put into practice.

Mr. Edmund Tattersall said, at an early period of his life he had suggested the simple plan of the rope and bell, which Mr. Allport had referred to as a means of communication between guards and drivers, but he did not imagine that the system would ever be allowed by the railway managers to be extended as between passengers and guards, although he always thought it necessary. He had considerable difficulty in getting the plan tried, but it was now carried out on nearly every line, and the Great Northern paid a small sum for the use of it for two years. It occurred to him that it was a disgrace to the present age, that mechanical talent could not invent some contrivance by which the desired means of communication could be obtained. He believed it to be quite possible, and he expected that it would shortly be carried out. Having been a competitor with Mr. Preece, he was ready to pay tribute to the ingenuity of the system described in the Paper, as far as he was capable of judging, not being an electrician, and to the zeal and ability with which the subject had been prosecuted. Mr. Tattersall had had similar facilities afforded to him by the Directors and Manager of the South Western Railway for carrying out experiments on their rolling stock; but his plan was reported to have failed in one particular when tried. He thought that Mr. Preece's system did not place the means of signalling sufficiently within the reach of all the passengers; but still he was ready to admit, having seen it in operation, that it possessed great merits. A similar system had, he believed, been prepared and tried by Mr. Woodifield.

Mr. Allport remarked that he had tried Mr. Tattersall's plan at least a hundred times during the journey from London to Derby, both in cuttings and on embankments, and with the train going fast and slow, and in every possible position, and he had failed in every instance to attract the attention of the guard. Mr. Tattersall was in error in speaking of the present system of communication between the guard and the driver as his own. An examination would convince him of his mistake.

Mr. F. J. Bramwell understood Mr. Preece to have drawn a distinction between the system of opposing currents and that system which he called equilibrium currents. He had gathered, from Mr. Preece's explanation of the diagram of the "opposing current" system, that there was employed at each end of the train a battery, and that the similar poles of those batteries were connected by the wire which extended from one end of the train to the other, whilst the opposite poles of the batteries were connected to earth. That this being the condition of things, no action took place until in some one of the carriages a connection was also made between the continuous wire and earth, and that then both batteries came into play. It was said there were objections to this system. The diagram which exhibited the equilibrium system showed, as Mr. Bramwell understood it and its explanation by Mr. Preece, a battery in every vehicle of the train, all the batteries having their similar poles coupled by a wire extending the whole length of the train, and their opposite poles all connected to earth; and it was stated that when in any carriage a communication was made between the connecting wire and earth, the batteries would go to work. From the subsequent explanation, however, and from the diagram, it appeared that the practice was not to put a battery in each vehicle, but merely in the various break-vans which might be expected to be detached with sections of the train at junctions; and that in a train which was not to be broken up into sections one battery only was used at each end of the train. With this condition of things he had failed to understand what it was that constituted the difference between the opposing system of currents and the equilibrium system, and fearing that others might have experienced the same difficulty, he thought he might be doing service by asking for an explanation.

Mr. N. Beardmore wished to ask, whether it was the fact that the train might be broken up at random, or whether there must always be a guard's van with a battery introduced to place the train in circuit: in other words, whether the tail of a train might be detached, and 'foreign' stock be joined on without failure of the apparatus.

Mr. S. A. Varley was desirous of explaining an electrical plan of train intercommunication, which had been fitted to the Royal Train on the London and North Western Railway, and which, in addition to experimental trips, had worked successfully on four journeys to Scotland and back.

The value of the telegraph, as an auxiliary in railway signalling, was now generally admitted; its application to train-signalling was a further development in the same direction. A railway train was in fact a section of the railway. Mr. Martin had, therefore, considered, that the system adopted should be assimilated, as much as possible, to that which his long practical experience had led him to consider the best.

Railway telegraphs might be divided into two systems,—one in which an audible signal was employed to call attention to a permanent visible signal, and the other in which an audible signal alone was depended upon. At the head of the former stood the system which had been adopted on, and been most completely developed by, the London and North Western Railway. Mr. Martin's might be considered as the London and North Western system of block signalling, and Mr. Preece's as the South Western bell system, as applied to trains. Mr. Martin had not had the advantage of trying a large number of experiments; but the system he had adopted for raising an alarm was nearly identical with that put forward by Mr. Preece.

Mr. Varley then proceeded to illustrate the working of the apparatus. In each compartment there was a lever-box, and when a passenger decided to raise an alarm; he pulled the lever handle, and in the act of doing so set the bells in all the vans ringing. To prevent repudiation of the signal, the lever when once pulled could not be replaced, until the guard came round and unlocked the box. So far the electrical arrangements of Mr. Martin's system were the same as Mr. Preece had lately adopted; but Mr. Martin's system did not stop here. The apparatus in each of the vans was furnished with three keys, on which were engraved "stop instantly," "stop at the next signal station" and "van bells." When the attention of the guards had been called, the guard in charge exercised his judgment whether to stop instantly or at the next signal station. Having decided, he depressed the key with the corresponding signal; the effect of this was to set a bell fixed on the weather-plate of the engine ringing, and also to deflect an indicator over to the signal legibly painted on a dial. After the guard had depressed the key by means of a peg, he kept it permanently depressed, and the engine-bell continued ringing, and the indicator to point to the signal, as long as the key was pegged down. When one of the guards wanted to call the attention of the rest, he depressed the key engraved "van bells," the effect of which was the same as when one of the lever-boxes was pulled. The advantages of Mr. Martin's system were, that an audible signal was used to give an alarm, and that the instructions were conveyed by permanent visible signals, about which there could be no misapprehension.

Mr. Spagnoletti, in describing the action of a system of train-telegraphing introduced by him on the Great Western Railway, remarked that there were few matters connected with the working of railways more publicly discussed than that of communication between passengers and guards and drivers of trains. A railway company not only dealt with the carriage of goods, &c., but was intrusted with the safety of the passengers. It was, therefore, necessary that a question of this kind should receive every consideration at the hands of railway companies; and in such a matter he did not think public opinion could be altogether disregarded. Any system of train signalling to meet the present emergency must be simple, strong, certain in action, and economical. In working out the plan he had introduced, he had given these primary requirements every consideration. It had been argued, that train communication was so seldom brought into practical operation, and the chances of its being used were so few and far between, that when required, any mechanical or electrical appliances were liable to be out of order. To this it might be replied that, on the Great Western and other lines, several instruments were employed, more intricate and delicate in their action, and not constructed with a view of such unfrequent usage, than that which he was about to describe; and no such contingency had arisen from their being seldom used, inasmuch as ordinary inspection was sufficient to keep them in proper working condition. It would be part of the guard's duty to test the apparatus before the train started, and if anything were wrong to report it, and to record any defects that might be noticed. In the system he had introduced there was an outside visible signal, as well as an electric signal. The apparatus he proposed to place in the passenger carriages was shown by a model which was described. It consisted, in each compartment, of a handle, placed over the carriage window, and secured by a pin with a chain attached; the object of the pin being to prevent the handle being moved by accident. To call the guard's attention, the pin had to be removed, and the handle to be turned, when a disc outside the carriage was brought into sight, and the bells in the guards' vans were rung by electricity, and if necessary a bell could also be rung on the engine, or a disc instrument could be placed on the engine, similar to that used on the Metropolitan Railway. The handle, when once turned, could only be replaced by the guard, who was provided with a key for the purpose. The coupling between the carriages was formed of an insulated wire cable, with a hook and eye at each end, so that whichever way the carriages might be turned, a connection could be made. The end of the cable was formed by the conducting wire running through a spiral wire. The spiral wire was stronger than the conducting wire, and the latter would be the first to break. In that case, any part of the train breaking away, the spiral wire would be pulled through the brass 'rose,' which was connected with the earth, and contact would be made by friction, which would give an indication to the guard that the train had broken away, and until the wire was removed, the bells would continue ringing. A small spring or plate was so placed in connection with the earth, that the hook resting on the spring would give notice to the guards if the train was not properly made up. There was another way of making the connection between the carriages, but as it did not come within the limits of economy so closely as the cable, he could not so strongly advocate it. This plan embraced an iron bar with a link at each end, to take the place of the present coupling chains, the electrical connection being effected by one link being tapped with a brass stud, to which a spiral wire was attached, so as to yield to the movement of the carriage; or by two springs pressing on it, so that contact was maintained by the wire under the carriage with the links at the other end of the carriage, and so on throughout the train. The end of the hook and eye was bossed with brass, so that there was a brass contact, which was kept clean by the oscillation of the train.

In another plan of giving the alarm, the disc was placed at the end of the carriage. Any person turning the handle, pulled a wire, which was passed through the curtain-rods of the carriage windows; this drew the bolt from the disc, and then it fell down into the position for attracting attention. The wire, being studded with pieces of iron, formed the electrical conductor as well. By that plan one disc answered for each carriage, and the handle being locked, showed the compartment from which the alarm was given. For a night signal, the disc fell upon a fuse, which was lighted by percussive action; or the fuse could be lighted by a friction string. On the Great Western the fuse burnt with a bright red light, illuminating the whole of the train for seven minutes. He had already alluded to the impression that instruments of this kind, when not frequently used, were liable to get out of order. To guard against such a result, two springs were placed parallel to each other, and on the iron rod an oval collar was so fixed, that when the handle was turned down, both springs were opened; in turning the handle, not only was contact made, but the springs were scratched, so that if one spring failed, the other was sufficient for the work. To meet the case of the local and suburban trains on the Great Western, for which composite carriages were used with a break compartment, the apparatus was made portable, so that the guard had only to take out the box as he would any other article, and replace it when required for another journey, the handle alone being left in the carriage as a fixture. This electrical communication was worked on what was called the equilibrium system, with batteries at each end of the train. It could be worked either in that way or by a single battery on the engine, or front van, and the communication was not interrupted by the manipulations of the train on the journey, and any number of carriages, slip coaches, or other vehicles, could be added or taken off the train without disturbing the remaining portion. He thought the outside signal, in addition to the electrical one, was an advantage, for if the bells failed, the disc would show the guards or drivers that a signal had been given, and it would also attract the attention of the station-master or switchman at any station the train might run through; and in cases of danger a message could then be sent to the station in advance, and the train be stopped. Royal saloons always had an outside signal affixed as an extra precaution. In this system he had studied to make the apparatus as complete, simple, and economical as possible, and he thought the cost of it was little in excess of the rope-and-gong system, besides being more effective and cheaper to maintain, for there was great wear of the rope, and, as it must be of the best quality, it was expensive.

At present his system must be regarded as experimental, as it had been fitted only to one train of three carriages; but the success was satisfactory in every way.

In connection with this another experiment was being tried: this was a cage, invented by Mr. Baker, so as to enable the guard to go from one part of a train to another while in motion; and when an alarm was given, he could at once see from which carriage and from which compartment it was given, by the disc being turned. The cage ran outside, on the off-side of the train.[1]

Mr. C. V. WALKER said, as the Telegraphic Engineer to the South Eastern Railway Company, he had for many years carried out a simple and efficient system of train signalling from station to station, which he had had occasion to bring before the Institution, during a discussion upon a Paper which had been read a year or two back.[2] The same description of voltaic batteries and signal-bells, that were used in signal-stations, were now placed in the trains in the guards' vans; with a proper and simple addition to the bells and batteries, to prevent their being affected by the motion of the train, and with apparatus to meet certain other conditions of the system. The arrangement, which had worked so well for a number of years in signal-boxes, was found by experience to work equally well in trains in motion. It was a simple plan, and one easy to keep in order. In the first instance, various experiments (or trial trips) had been successfully made, of which the public had been informed by the newspapers from time to time; and since the 12th of July, 1866, the system had been applied, by order of the Board of Directors, to the regular day mail trains between London and Dover,—namely, the 7·25 A.M. mail down, and the 3·45 P.M. mail up. These trains were regularly and permanently fitted with means of communication between passengers and guards, and guards and driver. Within the last few weeks, the same system had been applied to the 8·30 P.M. night down mail train, and the 4·30 A.M. up mail; and the requisite apparatus had been provided for an extension of the system to the other fast trains on the South Eastern line, in completion of the order made by the Board. He considered that simplicity was the chief recommendation of any plan of train-signalling; and that there was no greater difficulty in keeping an electrical system in order from end to end, in so short a circuit as a train, than there was between station and station, whether they were yards or miles apart. Hitherto the duty of coupling up the carriages had been performed by a telegraph clerk at each end of the line; but arrangements had now been completed for that work being done in the ordinary way by the shunters who made up the trains; and for this purpose detailed instructions had been issued.[1] A summary of the trips made, from July 12th to November 30th, showed that, out of 244 journeys, in 237 the communication was perfect with both guards, and in the remaining 7 cases, with one guard; and of these 7, there were 4 cases in which the train was started without being fully fitted with the apparatus. This was given in illustration of the ease with which an electric system travelling in a train might be maintained. These were not experiments, but the records of its regular use; and it had been working as well or better to the

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[1] Mr. W. J. Baker has since furnished the following particulars in reference to his Safety Cage, in which a guard can pass to any part of a train in motion, either in reply to an electric alarm, to make a periodical inspection of his train, or to collect the passengers' tickets, without stopping the train.

The 'Cage' consists of a strong framework of wrought iron, borne on grooved wheels which run between two parallel rails or guides, fixed for this purpose along the sides of the carriages, the upper rail being fastened to the outer edge of the roof, and the lower rail to the footboard or steps.

The couplings of the rails between the carriages are made to slide about twenty inches, to allow of the compression of the buffers, and have sufficient side play to prevent straining when the train is on a curve. The couplings are self detaching when the carriages are separated or 'slipped.'

A footplate, upon which the guard stands when he wishes to pass along the train, is fixed between the side-frames of the 'Cage,' and the outside is fenced in to prevent the guard from falling off: this fencing and the footplate are attached to the framework by joints or hinges, and can be folded close to the side of the guard's van or compartment when not in use.

To pass along the train, the guard takes his position on the footplate of the 'Cage,' facing the carriages, and pulls himself along by the handles or handrails of the carriages to any part of the train he requires to reach.

As the 'Cage' is kept folded by the side of the van when not in action, it is not available for improper or unauthorized use by any passenger, and the rails alone are of no service to afford a passage along the train.

[2] *Vide* Minutes of Proceedings, Inst. C.E., vol. xxii., p. 203, *et seq.*

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[1] SOUTH EASTERN RAILWAY.
INTERCOMMUNICATION IN RAILWAY TRAINS,
PASSENGERS AND GUARDS; GUARD AND GUARD; GUARDS AND DRIVER.

WALKER'S PATENT.

INSTRUCTIONS TO GUARDS, DRIVERS, AND OTHERS.

1. The Guards will have charge of the Electric Couplings, each Guard having one Tender Coupling and at least ten Train Couplings, and a box to contain them.
2. The Shunter or Porter, who makes up the Train, will couple up the Tender to the Train, and the Carriages to each other, before starting; and a Shunter or Porter will also uncouple and return the Electric Couplings to the box at the end of the journey.
3. The Head Guard will be responsible for the proper coupling of his Train before starting; he must, also, at the end of the journey, see that the Electric Couplings are deposited in the box provided for the same.
4. The Driver, or whoever separates Engine from Tender, will uncouple; and will recouple when he replaces them.
5. Each Guard will have in his Van at least four coils of insulated wire, for use when unfitted Carriages are added to a fitted Train. The Shunter or Porter will lead the wire over the roof of the unfitted Carriage, and twist tightly the bare copper ends on the spiral connecting hooks of the next fitted Carriages.
6. The **Switch** in each Van is to be set and **bolted** by the GUARD himself, as soon as the Train is coupled up for a journey. The Index in the **rear** Van is in all cases to point **downward.** The Index in all **intermediate** Vans is in all cases to point **upward.**
7. The Index in the **front** Van is to point **direct** to the Engine, when the Engine is **fitted** up; or to point **downward,** when an **unfitted** Engine is in use.

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8. The Front Guard has sole control of Engine Signals. With the Index set as in Rule 6, Signals reach the Guards only. The front Guard will set the Index (still pointing in the direction of the Engine), in the first notch of the upper brass segment, when Signals are to reach the Driver. All Signals made in the Train will then reach the Engine, ringing the Bell, but not raising the Semaphore Arm.

9. To raise the Semaphore Arm and ring the Engine Bell continuously, the **front** Guard will set the Index pointing **direct** from the Engine **to the Train.** The Arm may also be raised, and Bell rung if the Index is pointed upward, or is set intermediate between these positions. These last two positions are rarely required.

10. As the Train pulls up at the end of its journey, or at a Junction where changes are made, all the Guards will set their Indexes pointing upward. The Index to be kept in this position as long as Vans are not actually working.

11. The following are Guards' Code Signals, to be repeated back, and made by pressing the Knob of the Ringing Key:—

One Blow on the Bell	**Danger.—Stop.**
Two „ „	All clear, and **Look out.**
Three „ „	**Testing Signal.**

12. The Driver will hear Signals and Replies; he, and also the Guards, will look out in all cases, and he will pull up only when the Semaphore Arm is raised.

13. The Head Guard, before the fitted Train starts, will see that no Outside Signal Disc is open. All Guards will have Keys for replacing Bell-pulls and closing Discs. Each Guard will have with him at least half a dozen spare Bell-pulls.

14. When a fitted Carriage travels in an unfitted Train, the Guard of that Train, before starting, will remove all the Bell-pulls into his Van and open the Discs; and will replace them at the end of the journey. The Bell-pulls will be numbered and lettered. Fitted Carriages, not in work, are to stand with Discs closed and Bell-pulls in.

15. Should a Passenger withdraw a Bell-pull, the Bells in the Guards' Vans will continue ringing till the Train stops, and an outside Signal Disc will open and indicate the compartment from which the Signal comes.

16. Should a Passenger Signal be made, the Guards will act on their instructions as to stopping the Train. The name and address to be taken, in full, of any one who makes a false or idle alarm; failing this, of all Passengers in the compartment.

17.—Should anything appear defective on the Engine, the Driver will report to the Guard. Should anything be defective in the Train or Engine, the Head Guard will report to the Telegraph Department at the journey's end, who will advise the Telegraph Engineer. The Guard will also enter it in his Report of the Journey. Guards will see that the wires and all apparatus in their trains are not tampered with; but they are not required to interfere with the batteries or wires.

(*By Order*) C. W. EBORALL,
General Manager.
J. P. KNIGHT,
Out-Door Superintendent.

Superintendent's Office,
London Bridge *November*, 1866.

present day. The connection of the apparatus with the engine was not fully effected quite so early: the arrangements were made, but the fitting was deferred till the passengers were provided for. From November 1st to the 30th, when the engines were fitted, there were 50 cases—that was to say, every case in which a fitted engine ran, in which the communication with the engine was perfect: in the remaining two cases unfitted engines were used. As an illustration of the behaviour of the apparatus on the engine itself,—in the case of one engine, the apparatus was applied and adjusted to its work on September 15th, and had not been touched or examined up to November 30th, having been in working order for eleven weeks without being attended to in any way, and had worked equally well since. The same was the case with another engine for eight weeks, and with a third for seven and a half weeks, these also still being at work. He mentioned this as an illustration, in reply to suggestions that had presented themselves, that it was not so difficult a matter as those unacquainted with the subject imagined, to keep the apparatus in perfect working order, under circumstances of such apparent difficulty. The trains were fitted for public use; but there had been no occasion to use it. There was a bell-pull in each compartment, over which a card was posted, containing the following instructions in three languages, English, French, and German:—

"COMMUNICATION WITH GUARDS.

"When the Bell-pull is fully withdrawn, the Guards will be alarmed—will know whence the alarm comes—and will act on their instructions. If the Bell-pull is partially withdrawn, it shows a signal outside the carriage, which will attract attention at the next stopping place.

"Passengers cannot replace the Bell-pull; and they will be accountable for any false or unnecessary alarm."

In one instance a passenger pulled the handle in a carriage (in which there was not a card of instructions), as the up-train was coming out of Merstham Tunnel, and the bells rung, as it was arranged they should ring, all the rest of the way to London. It was done by inadvertence, and out of mere curiosity; there was nothing to indicate the object of the bell-pull. This occurred only a few days after the train commenced running, and before the guards had received instructions how to act. The passenger was greatly alarmed, on the arrival of the train in London, when taxed with what he had done. To ascertain that the couplings of the carriages were properly made was a simple operation, requiring only the most ordinary care and attention on the part of those whose duty it was to attend to it. There was a spiral or twisted hook at each end of each carriage and van; it was 8 feet above the rails, and was connected with a wire that passed round the carriage under the eaves of the roof. The coupling was a spring or spiral of brass wire, with a ring at each end, and enclosed, except the rings, in a tube of vulcanized rubber. The rings were put on the spiral carriage hooks, and the coupling was then complete. The engine was provided with a bell and semaphore. The bell could be rung either with or without raising the semaphore arm; in the former case, which was in the power alone of the head guard, the driver was required to pull up.

Mr. J. W. Bazalgette said, while admitting the ingenuity of the apparatus, it did not appear to him to strike at the root of the evil sought to be remedied—that was, the protection thus proposed to be afforded to passengers travelling by railways. To take the case of persons desiring to be protected against robbery or molestation in a carriage, it might be assumed, in the first instance, that the passenger intending to attack another would be possessed of the greater physical strength, and all he had to do was to place himself in front of the party he designed to attack, and prevent him giving an alarm by the signals. Then again, on the other hand, mischievously-disposed persons might stop a train unnecessarily, and do a great deal of injury. He would ask those who had practically considered the subject to say, how the difficulties to which he had alluded could be overcome, so as to make these means of communication of that practical use to the public, in cases of danger, which alone would justify any reliance on them.

Mr. J. P. Knight (Traffic Superintendent of the South Eastern Railway), said that Mr. Walker had to a considerable extent explained the details of the system of signalling in trains adopted on the South Eastern line as far as regarded the telegraph department. He would, therefore, confine his remarks to the operation of the system as regarded the traffic department. Mr. Walker had fitted this apparatus to between forty and fifty carriages, and to about five engines; and he would shortly hand over those trains entirely to the traffic department; he would then be relieved of his present responsibility with regard to the fittings and the coupling up of the trains, and the uncoupling at the end of the journeys. Instructions had been issued to the guards (pp. 102 and 103), embodying every detail necessary for carrying out the system efficiently. Copies of these instructions, mounted and framed, were posted in every guard's van; and, from the experience already gathered, he was quite sure, there would be no difficulty in the working of the communication. The guards would have the charge of the electrical couplings, which were stowed in the boxes in the vans at the end of the journey; and, besides the guards, the shunters, who coupled the trains before starting, would be responsible for the couplings being properly attached. The present provision, which the South Eastern Company had in work, furnished the passengers with the means of giving an alarm to the guards, and thence from the guards to the driver to stop the train. The moral and deterrent effects of such a provision were not to be disregarded, although, in cases of assault, it might not always be available.

The regulations referred to were now in force, and a copy of them was posted in each guard's van. Special instructions were issued as to the general circumstances under which a train should be stopped. There were parts of the line where it was inexpedient to stop a train—for instance, near a tunnel, or when approaching a station; but still no risk would be incurred by an unexpected stoppage between stations, as the South Eastern line was throughout its entire length worked on the block system.

Mr. Beardmore said, the question was whether, with the experience of the working of the system, the thing was worth the expense and trouble of providing it.

Mr. Knight replied that the results were satisfactory, or the company would not go to the expense of extending the system to other trains. It was, by order of the Board, about to be applied to the tidal trains, and it would no doubt in due time be extended to the principal express trains. He could not say that it would be applied to the omnibus or local trains, where the stoppages were so frequent, and for which a different class of carriage was employed. In the latter case it was very rare that a passenger was alone, or with only one other passenger in a compartment in the neighbourhood of London. It was not proposed to apply the system to other trains than those going long distances without stopping, such as the ones before alluded to. He thought the public would feel a great degree of satisfaction with these arrangements. They would know that there were means at their command to give an alarm if occasion arose. It might very seldom occur. Indeed, in six months there had been only one instance of an alarm given on the South Eastern Railway, and that was by a soldier; but the guard, after having ascertained that it was an uncalled-for alarm, exercised a wise discretion, and did not stop the train.

Captain H. W. Tyler would first address himself to the *cui bono* question, which was at the root of the whole matter. If there was no practical utility in this sort of apparatus, it was certainly not desirable to go to the expense of providing it. He considered the object was not to prevent persons from being suddenly knocked on the head. No apparatus whatever could altogether prevent such a crime as that from being committed. It might as well be expected to organize a system of telegraph from each foot-passenger in Great George Street to the nearest policeman, to prevent his being garotted. Nor was it practicable, with the existing form of carriage, altogether to prevent the possibility of insult or assault by the strong upon the weak, or to do more than provide a deterrent influence in such cases. The principal object to be attained, in establishing intercommunication in trains, was to provide against danger to the public in cases of carriages catching fire or leaving the rails, or an axle or a tire breaking, or any of those other occurrences which, though they might not often happen, were always attended with considerable danger. He thought the subject might be simplified by dividing railway traffic under two heads, omnibus traffic and express traffic. The cellular system, which had been chiefly adopted in this country—from the old stage-coach—in the construction of railway carriages, and which had been continued over a great part of the Continent, was, in some respects, most unfortunate. It answered comparatively well for those trains which made frequent stoppages; and it possessed advantages in the interests of the companies for omnibus traffic, as the greatest number of people could be loaded into each carriage. By the use of side doors the passengers could get in and out with rapidity; and three different classes of passengers could be accommodated in the same vehicle. But the cellular system was not so well adapted for trains which ran long distances and made few stoppages. In those trains passengers were not so closely packed, or so frequently changed; and it was in those trains that a means of communication with the servants of the companies was more particularly required. Those trains were formerly run with the same carriages as the ordinary trains, but, latterly, railway companies had wisely adopted carriages of superior construction for the long-distance and non-stopping trains; and he thought it unfortunate that a different system of internal division had not at the same time been adopted. A passenger travelled from Charing Cross to Dover, a distance of 88 miles, or from Chester to Holyhead, a distance of 84 miles, or from Euston to Rugby, a distance of 82½ miles, without stopping, not knowing, when he started, who might be his companion for two hours, or what might happen in the carriage, or to the carriage, in the course of those two hours during which he was to be without any means of communication with the outer world. He need not go over the many instances that had happened to show the want of communication, but he would specially refer to four. The first case which drew attention prominently to the subject was one which occurred on the Midland Railway, in 1847, to Lady Zetland, whose carriage was set on fire by a spark from the engine. And the latest illustration had also been provided on the Midland Railway, where a train caught fire in which he believed Mr. Allport was travelling. He was informed that was only a day or two after Mr. Allport had said how easy it was to stop the Midland trains, and how little necessary it was to adopt any better system of signalling in trains. The two other cases to which he referred occurred to Members of this Institution, and both on the Great

Western Railway, in March, 1865. On the 21st of that month Sir Charles Fox was travelling up from South Wales with a brother engineer: he was suddenly awoke, at half-past three o'clock in the morning, near Goring, by the fracture of the tire of the wheel over which he was sitting. The passengers in the other compartments of the carriage joined with him in a vain chorus of shouting. The front guard was unaware of what had happened, and heard nothing. But a guard in the fifth, and another guard in the seventh and last carriage, felt a shock from running over portions of the tire or wheel upon the rails. They vainly endeavoured, for 6 miles, to attract the engine-driver's attention by waving their hand-lamps, and by suddenly applying and releasing their breaks, though they heard nothing of the shouts of the passengers. The engine-driver thought the train went heavily through the Pangbourne cutting, and gave his reversing lever another notch, under the belief that the wind was strong against him. Looking round afterwards to ascertain whether all was right, he could see nothing wrong with any of the carriages, and was only induced at length to pull up by noticing the hand-lamp of one of the guards. On the 24th of the same month Mr. Baker, the Engineer-in-Chief of the London and North Western Railway, travelled with his wife and a fellow of Trinity College, Cambridge, in the same carriage from Leamington to London. The train was composed, when they left Reading, of an engine and tender, a break-van, and four carriages. They rode in a composite carriage behind the leading van. An axle of that carriage gave way shortly after the train left Reading, and they were bumped and jolted along for the whole distance of 34 miles to London in that condition. The different passengers in the carriage waved hats, coats, rugs, and handkerchiefs out of the windows, swung the doors backwards and forwards, shouted, and did all they could to attract attention. Mr. Ransford, who rode in the last compartment of the same carriage, shook his handkerchief, as he said, frantically in a signalman's face, as he passed him. But the signalman saw nothing wrong. Mr. Baker's arm was broken by being struck against a signal-post at the side of the line in the course of his exertions to attract attention, and Mr. Ransford's hand was severely injured in the same manner. But neither the engine-driver, nor the two guards, nor the numerous switchmen whom they passed, became aware of anything being wrong. Fortunately, it was only as the train entered the Paddington station that the carriage left the rails, and no further injury, beyond a severe shock to the nerves of the passengers, was sustained. It would, however, be worth while to read the description of Mr. Baker's feelings on that occasion, as given by him when he took his evidence officially after the accident. Mr. Baker said: "We had thus upwards of half an hour of awful suspense, during which we expected every moment to be dashed to pieces; and I may add, that I most seriously regretted the absence of any means of communication with the guard, so that he might stop the train and release us from our perilous position." The question was, then, whether passengers being at all times liable—though they were, he admitted, of comparatively rare occurrence—to accidents of this description, it was not worth while to provide some apparatus to be used in case of need by the passengers for those trains which ran long distances without stopping? He thought, himself, that the running of such trains without some provision of this sort was indefensible, and his opinion was confirmed by the higher authority of the report of the Sub-Committee of General Managers, who reported on this subject from the Clearing House in March, 1865. Mr. Forbes was the Chairman, and Mr. Allport, Mr. Seymour Clarke, Mr. Cawkwell, Mr. Eborall, Mr. Grierson, Mr. Moseley, and Mr. Archibald Scott were Members of that Sub-Committee. They stated in their Report as follows:—"The Sub-Committee is, however, of opinion, that it is desirable, if practicable, to give passengers by express or other trains running for a considerable distance without stopping, the means, in cases of emergency, of attracting the attention of the guard, and of enabling him to stop the train at the next station or under the protection of the next fixed signals; and they recommend that no effort should be spared on the part of the Railway Companies to attain this object, it being borne in mind that, in order to give the public the full advantage of the communication upon the long through trains, many of which traverse several lines of railway, it is absolutely necessary that all the Companies should adopt the same plan." That was the deliberate opinion of the eminent railway managers whose names he had mentioned, and he thought that it at once disposed of the *cui bono* question. It being held, then, on such high authority, that it was desirable to adopt some means of communication in trains, the next question was, which was the best means of effecting it? He had examined great numbers of inventions of different descriptions, and had come to the conclusion that the voltaic current was, upon the whole, the best means that could be employed. Mr. Preece was early in the field, and deserved all praise for the labour and attention he had given to the subject. Captain Tyler feared, for some time, that there would be no rivals, as other electricians were so long coming forward. He was happy to find there were now others in the field equally eager to adopt something of their own, and he hoped, out of this array of electrical science, some bright sparks would yet be evolved. The experience in this country of this matter was, as yet, limited to what had been done on the South Western, the South Eastern, the Great Western, and a few other lines; but the experience which had been obtained in France extended over a much greater area and a longer period. The only difficulty, but the most important problem in all voltaic apparatus for this purpose was, the mode of coupling between the carriages. That which was exhibited by Mr. Preece had received ample trial on the Northern Railway of France, and had been used with such success, that the French Government had invited all the railway companies to adopt the same system. It was now in use on almost all the lines in that country, as a means of communication between guards and drivers. It had worked so satisfactorily for that purpose, that the system was being further adopted for the use of the passengers.

There were many who considered it was better to enable the passengers to communicate with the guards only, and not with the engine-drivers. But he thought it more important, on the other hand, that the passenger should be able at once, when anything was wrong, to attract the attention of the engine-driver. It was the driver only who had, and must have, the control of the train while it was in motion. And it was of extreme importance, in the case of such an occurrence as the breaking of an axle or a tire, or of a carriage leaving the rails, that the driver should have the earliest possible intimation, so as to enable him to act according to the best of his judgment. It did not, by any means, follow that whenever a passenger made a signal the train was to be immediately brought to a stand. It was not always expedient to stop a train suddenly. Such a course would, under some circumstances, be attended with the worst consequences. But when the attention of the driver was awakened to any mishap, he would shut off the steam, and slacken the speed suddenly or gradually, as appeared to him best, so as eventually to stop the train in the manner best suited to each particular case. Absolute perfection could not be expected in this, any more than in any other apparatus. Neither the engines nor the carriages were perfect; but that was no reason for not using them. All that could be expected was, that it should be reasonably good, and that system could only be adopted which appeared to be best adapted to the object in view.

But, after all, the question of communicating between different parts of a train while in motion was, in his opinion, less important than that of circulation; and when he spoke previously of the construction of express-train carriages, he could not but feel, that if means of passing through the trains from end to end on these long journeys were available, much of the evil that at present existed would at once be avoided. It was principally in consequence of the want of the means of circulation, that so much apparatus of communication was required. There were two ways of providing circulation in a train — external and internal. The limited space that existed on many of the railways between the carriages and the works at the side of the line rendered external circulation out of the question. He had expected to hear something more of the cage, about which Mr. Spagnoletti had spoken as a means of external circulation. But after having gone carefully into that subject, he had found that there was, in many cases, no room between the sides of the carriages and the works not only for a cage or a protected passage, but even for a man to pass with safety. And even if there were sufficient space, he thought that such a means of circulation would prove to be highly objectionable in this country, for several reasons. It would afford opportunity for access from one part of a train to another to persons by whom passengers would not wish to be visited, and this would be particularly objectionable in night trains. Few passengers would sleep comfortably if they knew there was a gallery outside along which villains might travel, and the passage by which could not be effectually controlled at all parts of the journey. The murderer of M. Poinsot was believed to have entered the carriage and escaped from it along the footboards. In the case of internal circulation, it would, on the other hand, be easy to provide against improper intrusion. The guards of the train would themselves circulate safely, would control the passengers effectually, and would be able to afford any relief that might be required. The practical question was this:—Was it possible and desirable in those trains which only stopped at long intervals to adopt some means of communication between the passengers and the servants of the companies? He did not hesitate to answer that question in the affirmative, and to add, that circulation was even more required than mere communication. He thought also, it would be a great benefit if, in connection with improved carriages adapted for through-circulation, sleeping accommodation were provided in the night trains between England and Scotland, and between London and Holyhead. When he was in Italy, considering the question of the Italian route for the Eastern mails, he proposed to the Government officers and the officers of the railways to provide sleeping-carriages for the long journey through that country. He was at first met with the remark, that the English, the most enterprising people in Europe, and the foremost in railway improvement, had, as yet, provided no good means for sleeping in trains. But the Italian government afterwards consented to supply sleeping-carriages for the journey with the Indian mails to Brindisi at moderate charges. He submitted that on long night journeys like those from London to Edinburgh, or to Aberdeen, it would be a great boon to travellers to be able to sleep comfortably on the road. They could do so on American, Indian, and Russian railways; and the French sleeping *coupés,* at an extra charge, were much in request. But in that respect we were at present woefully behindhand; and he thought it a subject worthy the attention of railway managers, in connection with that of providing communication and circulation in railway trains, with carriages better adapted as regarded convenience and safety for long journeys.

Mr. J. W. Bazalgette said, no doubt all would admit, that it was desirable to have communication in trains between guards and passengers; but at the same time, it must be allowed, that the system of signals at present provided, or recommended, did not secure the personal safety of the passengers in case of sudden attack. The causes which gave rise to these inventions were not trains catching fire, as such instances were the exception, but the more common danger to unprotected passengers. There could be no question it was to provide for the personal safety of the passengers that these inventions were first proposed; and he submitted, if that object was not effectually secured, these inventions were up to this time failures. He thought it most desirable that attention should be directed towards providing against the more common sources of danger, rather than the exceptional ones, so that the public might not be resting their safety, in cases of emergency, upon a broken reed.

Mr. T. E. Harrison, as one of the original Committee, to whom it was remitted by the authorities of the Clearing House to report on the subject of communication between guards and drivers and between passengers and drivers, would state that that Committee came to the conclusion, after the examination of a great number of plans, to adopt that which had been generally in use ever since as a means of communication between guards and drivers, the simple rope and bell, worked by the guard from his van. It had been said that was an expensive plan, but he did not regard it as such; it was simply a question of wear and tear of rope; and the pounds, shillings, and pence part of the question was an exceedingly small one. As a means of communication between guards and drivers, he believed it to be by far the simplest and most effective that could be adopted, and infinitely preferable to electric communication. He might state that the apparatus by which this mode of communication was now carried out was identical with a model submitted to that Committee fifteen years ago, by Mr. Fletcher of the North Eastern Railway, under his directions; the model of the bell being made at the Great Northern Railway Company's shops. This was afterwards used on the Great Northern and other lines. When the report, drafted by Captain Huish, was submitted, Mr. Harrison urged upon the Committee that, instead of the rope being carried on a level with the foot-boards of the carriages, it should be passed along the roofs of the carriages, so that any person could easily get hold of the rope from the inside of the carriage, and communicate direct with the driver. The opinion, however, entertained by the

Committee at that time was, that it was not desirable to have direct means of communication between the passengers and the driver, and for that reason, the rope had been used ever since by being passed along at the level of the footboards. Travelling so much as he did, he had met with a great number of casualties. He had been in several collisions; engines had run off the line, and on one occasion he had travelled with a madman; and he did not believe in any of those cases the communication between the passengers and the drivers would have been of the slightest use. He agreed with Captain Tyler that in cases of accident it was more important for the passenger to be able to communicate with the driver direct than with the guard, because the communication to the former ought to be as instantaneous as possible; but he believed the cases in which such communication might be useful were chiefly confined to those which had been mentioned,—fire, and the breaking of an axle or tire. During the fifteen years that the rope-and-bell system had been in use on the east coast, only two or. three instances occurred in which it was found to be of any use at all—one being in a case of fire. He thought any elaborate system of communication between passengers and guards to meet any imaginary cases, such as those of robbery or murder, would be utterly and totally useless; and if only attention were paid to cases of fire, the breaking of an axle, or any casualty occurring to the carriage, that provision would practically be made for all those cases for which it was necessary. He believed, if it was desirable to effect communication between passengers and the engine-man, instead of all these elaborate arrangements, which might not always act when wanted, it might be done by following out the simple rope-and-bell communication, as it was now in use, the rope being carried along the roofs of the carriages, instead of at the level of the footboards. He was afraid railway companies were going to a large expense in fitting electric apparatus to the trains, and in his opinion a great amount of ingenuity had been exercised in providing for an evil which did not exist.

Mr. W. H. Preece, in reply upon the discussion said, he had inquired of various railway companies the cost of carrying out the rope communication between guard and driver; and he found that on the Great Northern, a length of 42 miles of rope was consumed in ten months, at a cost of £187; in addition to which men were employed to look after it. He believed the cost for this bell-and-rope communication on the Great Northern lines was not less than £500 per annum; and he had no hesitation in saying that his electric system could be maintained on that line at considerably less cost.

He would not enter into the *cui bono* question, because that did not form part of his Paper, but, passing to the discussion in general, he might observe that the electric system of communication he had brought forward was not an experimental one. It was now a pure matter of fact, inasmuch as it had been practically carried out on the South Western Railway for more than two years, had been in use on the Midland for seven months, and also on the Great Northern Railway Companies' trains to Inverness for more than eight months. The general result was, as far as the question of carrying out the means of communication between passengers and guards, guards and guards, and guards and drivers by means of electricity was concerned, that it was a comparatively simple and economical matter. Neither its first cost, nor the cost of maintenance was great; in fact, on the whole working of a railway, there was no part of the general management which was likely to cause so little trouble or expense as an electric communication between passengers and guards. Mr. Allport stated that this system had been worked on the Midland line and had been abandoned. It was arranged by the Committee of Managers that they should select the most promising system and try it. The Great Northern and Midland took up this system, and trains were fitted with the apparatus, but not under his control. They ran for six or seven months entirely out of his control, and to the present moment he could not get a report from the Midland as to the results of the working. He believed, however, that during the six months the apparatus was in working, it got out of order only three times; and that it was eventually abandoned, not from the failure of the apparatus, but from the difficulty of keeping a few exceptional carriages together upon such a large system as that of the Midland. He thought any system, mechanical or otherwise, that would continue in perfect order for that period without attention, must be regarded as thoroughly practical and efficient. It had been mentioned that, on one occasion, the bells started ringing and could not be stopped till the train reached London. That might be true; but he pointed to it as one of the safeguards of the system, because in the ordinary rope-and-bell system, if the rope became deranged in time of danger, no alarm could be given. In his system, however, if anything went wrong with the apparatus it gave tongue at once, and it was obliged to be put in order before it could be silenced. It was admitted that the system worked well on the South Western, although after completing the experiments but little personal attention had been given to it. The whole thing had been handed over to the Traffic Department under Mr. Archibald Scott. If the system worked well on one line, there was no reason why it should not work well on every line.

It had been asked of what use was the apparatus, seeing it was so seldom used? But it must be recollected that before a system of this kind had been established there was a succession of occurrences which awakened the mind of the public to the necessity of providing as far as possible against their repetition—and since then there had been nothing of the kind. It must therefore be conceded that at all events, the introduction of the system had had some deterrent effect. It would, of course, be preferable that no necessity should ever arise for calling the apparatus into action, and he believed the deterrent effect was one of the greatest evidences that could be produced of its efficiency. The several systems alluded to during the discussion were all modifications of the principle he had propounded, the system of electric equilibrium; but with, what he considered, needless accessories.

He would say, in conclusion, as a practical electrician, although he might give a prejudiced opinion, he saw no difficulty whatever in carrying out this, or any electrical, system. He did not think the public, the press, or parliament would be contented without something of the kind. He had, however, simply performed his duty as an Engineer to those who had employed him, and had endeavoured to mould that force of Nature with which he was best acquainted to carry out those requirements which he had been called upon to fulfil.

Mr. C. H. Gregory, V.P., in closing the discussion, remarked that Mr. Preece, a very able electrical Engineer, had evidently devoted considerable attention to the subject he had brought forward; and the system he had worked out for establishing communication between passengers and guards appeared to have succeeded on the South Western Railway. However, one slight failure had occurred in the apparatus erected in the room, and worked by the able and careful hands of Mr. Preece himself; and it would probably occur to those present, that if certainty of action could not be secured under such favourable circumstances, failures would be still more likely to occur when the connections had to be made in actual practice on railways by the rougher hands of railway porters. He had no wish to disparage the beautiful mechanism that had been brought before them; but he thought it was a serious question, whether any apparatus, however ingenious, whose delicacy rendered it liable to failure, might not by giving false confidence be a cause of danger at the critical moment. As far as experience went at present, it seemed that any such system was rarely called into operation, and its functions would appear to be mainly to deter people from crimes in railway trains. But, assuming efficiency in the details of any system, the practical question would still remain, whether its adoption was necessary and generally desirable? What he might say on this question was so much better expressed in the Report of the Sub-Committee of the Railway Clearing-House in 1865, that he would read the portion of that Report preceding the extract which had been given by Captain

Tyler, and which more fully conveyed the views of the Committee on the general question submitted to their consideration. It was as follows:—

"12. Upon the more important question as to whether in the interest of the public safety the means of communicating with the guards should be placed at the disposal of the passengers, the following extract from the Report of 1853 expresses so accurately the objections to such an arrangement that this Sub-Committee cannot do better than give it a place in this Report. It is as follows:—'When discussing and weighing the inferences naturally deducible from the facts and information which it had collected, the Committee gave its attention, in the first instance, to the important question of giving to passengers the power of communicating with the guard. Without overlooking the possibility of such an arrangement being occasionally of service, the Committee have been unable to persuade themselves that it would not lead to greater disasters than it could, on any view of the matter, prevent. Unless the guards and engine-drivers had orders to stop the train whenever a passenger made a signal, the privilege would be useless to the latter. It however requires little acquaintance with railway travelling to be convinced that its dangers would be greatly increased if the train were to be stopped wherever and whenever a passenger, under the influence of fear or levity, chose to make a signal.'

"13. The views expressed by the Clearing House Committee in 1853 were adopted by the French Railway Commission of 1861. That Commission was composed of many eminent men, among whom were members of the French Government and Legislature, and engineers of the highest standing, and it was presided over by M. Michel Chevalier. These gentlemen, appointed by the government conducted an inquiry upon a matter as to which the public feeling was strongly moved by the murder of M. Poinsot, and not in any way interested in the question as regarded either the convenience, cost, or responsibility of the Railway Companies, felt it their duty from the very first to discard all idea of giving the passenger any power of communication, and they reported as the result of their inquiry into this subject, that such a communication would involve greater danger than advantage. The members of the present Sub-Committee, having given the matter much attention, and having anxiously endeavoured to find some efficient plan for effecting a communication between passenger and guard, are not able to come to a conclusion essentially different from that resulting from the preceding inquiries both in England and France. Nor can they satisfy themselves that this communication is generally desirable or could safely be allowed, except in very special cases, and then only under a stringent law controlling its use by the public. The fact of it being practically impossible that a guard, upon receiving a signal from a passenger, can go at once to the assistance of that passenger, materially limits the use and value of such a communication, for not even the most strenuous advocates for furnishing it can contemplate that the guard in charge of a train should, upon receiving a signal, and without being in a position to judge as to the urgency of the case, then and there stop the train. The inconvenience and positive danger resulting from such a course are too obvious to need further comment.

"14. Upon a general review of the information and facts obtained by the present inquiry, the Sub-Committee is obliged to report that none of the means of communication submitted to it are, in their present condition, such as could be relied upon and recommended for general adoption, and that in no case would it be desirable, even if possible, to establish a means of external communication by footboards."

He had thought it due to the Committee, as none of them were present to speak for themselves, that their general conclusions should be recorded, and these conclusions would be entitled to consideration as coming from some of the leading managers of railways. Whatever might be the views of the Meeting on the general question, he believed all would agree that Mr. Preece's Paper was an interesting and valuable one, and had well earned the thanks of the Institution.

Mr. W. H. Barlow asked permission to read a short extract from a note he had received from Mr. Allport on the subject of the rope-and-bell system of communicating in trains. Mr. Allport wrote:

> "Prior to 1858, the cord and bell had been tried upon four trains; and in October of that year the system was applied to all the passenger engines, carriages, horse-boxes, vans, &c., at a total cost of nearly £1,500. The communication has been in operation on 575 trains daily since 1858, or 189,139 trains yearly; and during the whole period it has been used only three times. In one case a fire occurred in the train, which, however, the driver had observed before his attention had been called by the guard. The second case was merely a hot axle. The other was the breaking down of a carriage, which happened close to a station where the train had to stop. So that in eight years, with upwards of 1,500,000 trains, it had only been twice useful."

ANNUAL GENERAL MEETING.

December 18, 1866.

CHARLES HUTTON GREGORY, Vice-President,
in the Chair.

Messrs. H. Hayter, T. Marr Johnson, R. C. May, T. M. Smith, and F. C. Stileman, were requested to act as Scrutineers of the Ballot, for the election of the President, Vice-Presidents, and other Members and Associates of Council; and it was resolved that the ballot papers should be sent for examination, at intervals of fifteen minutes, in order to expedite the labours of the Scrutineers.

The list of members prepared for Council, together with the record of the attendances of the Members of Council in Council and at the Ordinary General Meetings was read, and the Ballot was declared open.

It having been stated that certain charges had been made against a Member of Council, it was moved and seconded,—

That the subject be referred to the Council for their investigation and report. To which the following amendment was moved and seconded,—

That this meeting be adjourned until the 8th of January, 1867.

On the amendment being put from the Chair, it was declared to be lost, and the motion was put and carried.

The Annual Report of the Council, on the proceedings of the Institution during the past year, was read.

Resolved,—That the Report of the Council be received and approved; and that it be referred to the Council, to be printed and circulated with the Minutes of Proceedings, in the usual manner.

Resolved,—That the thanks of the Institution are due, and are presented to Messrs. F. W. Sheilds and Geo. P. Bidder, jun., for the readiness with which they undertook the office of Auditors of Accounts, and for the clear statement they have laid before the Meeting; and that Messrs. Geo. P. Bidder, jun. and J. D. Baldry, be requested to undertake the office of Auditors for the ensuing year.

Mr. Bidder, jun. returned thanks.

The Telford Medals, and the Telford and Manby Premiums of books, which had been awarded, were presented.

Resolved,—That the thanks of the Institution are justly due, and are presented to the Vice-Presidents and other Members of Council, for their co-operation with the President, their constant attendance at the Meetings, and their zeal on behalf of the Institution.

Mr. Gregory, V.P., returned thanks.

Resolved unanimously,—That the cordial thanks of the Meeting be given to Mr. Fowler, President, for his strenuous efforts in the interests of the Institution, for his extraordinary attention to the duties of his office, and for the urbanity he has at all times displayed in the Chair.

Mr. Gregory, V.P., returned thanks, in the unavoidable absence of the President.

Resolved unanimously,—That the cordial thanks of the Meeting be given to Mr. Charles Manby, the Honorary Secretary, and to Mr. James Forrest, the Secretary, for their constant zeal and devotion to the interests of the Institution, the ability displayed by them in the execution of their duties, and their attention to the individual wishes of the members.

The Ballot having been open more than an hour, the Scrutineers, after examining the papers, announced that the following gentlemen were duly elected to fill the several offices in the Council for the ensuing year:—

President.

JOHN FOWLER.

Vice-Presidents.

Joseph Cubitt.
Charles Hutton Gregory.
Thomas Hawksley.
John Scott Russell, F.R.S.

OTHER MEMBERS OF COUNCIL.

Members.

James Abernethy.
William Henry Barlow, F.R.S.
John Frederic Bateman, F.R.S.
Nathaniel Beardmore.
James Brunlees.
Thomas Elliot Harrison.
George Willoughby Hemans.
John Murray.
George Robert Stephenson.
Charles Vignoles, F.R.S.

Associates.

Lord Richard Grosvenor, M.P.
Charles Thomas Lucas.

Resolved,—That the thanks of the Meeting be given to Messrs. Hayter, Johnson, May, Smith, and Stileman, the Scrutineers, for the promptitude and efficiency with which they have performed the duties of their office; and that the ballot-papers be destroyed.

A vote of thanks to Mr. Gregory, V.P., for his conduct in the Chair, was passed by acclamation.

ANNUAL REPORT.

Session 1866-67.

Having been intrusted, during the last twelve months, with the direction and management of the affairs of this Institution, the duty now devolves upon the Council of submitting a Report upon its present state.

Before proceeding to allude to those matters which have more immediately occupied the attention of the Council in their private meetings, it may be well very briefly to refer to the Papers which have been read, and to the discussions which have taken place, at the Ordinary General Meetings. The Council have not failed to notice the increased interest felt in the meetings by the members of all classes, and they infer from this, that the subjects selected for consideration have met with approval. It should be observed, that although, from the length to which the discussions have extended, it has only been possible to bring forward a limited number of Papers, yet that others of equal merit have been thus unavoidably postponed. It is but fair to the Authors of these deferred communications to mention this fact; and, no doubt it will, in every succeeding year, become more and more difficult, so to limit the discussions as to allow of all the Papers being dealt with which have been accepted by the Council.

During the last Session there were twenty-four Ordinary General Meetings, when ten Papers were read, all of which have been deemed by the Council worthy of reward. The very full attendance at the meetings, and the fact that the Minutes of Proceedings relating to them have been issued, render unnecessary any lengthened notice of the contents of the Papers, or of the observations to which they gave rise. It will have been remarked that, as a rule, the communications, both written and oral, were accompanied by an unusual elaboration of detail, and a profuseness of illustration, testifying at once to the care with which they had been prepared, and to the desire to sustain the character of the proceedings. The Paper on Permanent Way, by Mr. R. Price Williams, M. Inst. C.E., may be cited as an example; and the encomiums, so deservedly passed upon it at the time when it was discussed, will be fresh in the recollection of the members.

For these communications the following Telford Medals, and Telford and Manby Premiums of Books, have been awarded:—

1. A Telford Medal, and a Telford Premium, in Books, to Richard Price Williams, M. Inst. C.E., for his Paper, "On the Maintenance and Renewal of Permanent Way."
2. A Telford Medal, and a Telford Premium, in Books, to John Grant, M. Inst. C.E., for his Paper, "Experiments on the Strength of Cement, chiefly in reference to the Portland Cement used in the Southern Main Drainage Works."
3. A Telford Medal, and a Telford Premium, in Books, to Edwin Clark, M. Inst. C.E., for his Paper, "On the Hydraulic Lift Graving Dock."
4. A Telford Medal, to Sir Charles Tilston Bright, M.P., M. Inst. C.E., for his Paper, "On the Telegraph to India, and its Extension to Australia and China."
5. A Telford Medal, and the Manby Premium, in Books, to Robert Manning, M. Inst. C.E., for his Paper, "On the Results of a Series of Observations on the Flow of Water off the Ground in the Woodburn District, near Carrickfergus, Ireland; with Rain-gauge Registries in the same locality, for a period of twelve months, ending 30th June, 1865."
6. A Telford Premium, in Books, to William Humber, Assoc. Inst. C.E., for his Paper, "On the Design and Arrangement of Railway Stations, Repairing Shops, Engine Sheds, &c."
7. A Telford Premium, in Books, to George Rowdon Burnell, M. Inst. C.E., for his Paper, "On the Water Supply of the City of Paris."
8. A Telford Premium, in Books, to William Ridley, Assoc. Inst. C.E., for his Paper, "On the Grand River Viaduct, Mauritius Railways."
9. A Telford Premium, in Books, to Theodore Anthony Rochussen, Assoc. Inst. C.E., for his Paper, "On the Maintenance of the Rolling Stock on the Cologne-Minden, and other Prussian Railways."
10. A Telford Premium, in Books, to William Hemingway Mills, M. Inst. C.E., for his Paper, "On the Craigellachie Viaduct."

It is with peculiar satisfaction the Council are enabled to report that, during the recess, in accordance with a promise made at the last Annual General Meeting, all the volumes of the Minutes of Proceedings that were in arrear,—*viz.*: Vols. XXII., XXIII., and XXIV.,—as well as Vol. XXV. for the last Session, have been issued to the Members. These four volumes contain 666, 540, 564, and 555 pages, and 24, 11, 28, and 27 plates respectively, or together 2,325 pages of letter-press, and 90 plates, besides numerous woodcuts. For this result, your thanks are due to all the officers and servants of the Institution, for all have zealously endeavoured, in their several departments and to the best of their ability, to carry out your wishes. In recognition of these services, the Council have placed upon their Minutes the following:

Extract from the Minutes of Council, October 10th, 1866.

"Resolved—That the best thanks of the Council be given to Mr. Manby, the Honorary Secretary, for his kind and valuable co-operation in bringing up the arrears of the Minutes.

"The Council desire to record their appreciation of the great exertions which have been made by the Secretary, Mr. Forrest, since the close of last session in bringing up all arrears of the publications of the Minutes of Proceedings of the Institution to the present time, and have resolved to present to Mr. Forrest a cheque for One Hundred Guineas."

In future it is proposed to issue the Annual Volume in quarterly parts, to those members who desire it, and to complete each volume before the commencement of another Session.

In reference to the Resolution passed at the last Annual General Meeting, as to providing additional accommodation for carrying on the business of the Institution, and which was again remitted to the Council, after the Special General Meetings of Members and Associates held on the 5th and the 26th of June, 1866, the Council still have under their earnest consideration the question of the best and most feasible means of providing additional accommodation for the Meetings and other uses of the members of the Institution, and they are engaged in the investigation of the various plans which have been suggested for that purpose; but they are of opinion, that it would be premature to enunciate any definite views in the temporary absence of the President, of whose further advice and assistance the Council are anxious to avail themselves.

As you are aware, it was resolved, at the Special General Meeting on the 26th of June, that the Subscription List of promised contributions to the proposed new building should be cancelled. Mrs. Locke, however, requested that her donation of £200, which had been previously paid, might be retained until the building should be proceeded with. If the intention to build be given up altogether, then Mrs. Locke leaves the appropriation of the money to the Council for the time being. The Council have tendered their thanks to Mrs. Locke for this gift, and the amount has been placed on deposit at the Union Bank, for the present to a New Building Fund account.

In the belief that it is desirable to render the Institution as useful as possible to the younger members of the profession, various measures have been discussed in the Council during the last session, which, it was thought, were calculated to promote this object. No complete scheme has yet been matured by the Council, but as one step in this direction, it was resolved that, from and after the 1st of March last, the Library should remain open until 10 p.m. every week-day, for the convenience of the members generally. It was also decided that, under such rules and limitations as might from time to time be found necessary, tickets of admission to the Library, available for any period not exceeding three months, should be issued to the pupils and assistants of the members of all classes, on a written application to the Secretary from any member, giving the name and address of the pupil or assistant, and stating the time for which such admission was desired. It must be added that, so far, this privilege has only been taken advantage of to a limited extent, and it will be for the incoming Council to consider whether the arrangement should be continued, with or without modifications.

Recently, the Council received, with much interest, a Memorial, of which the following is a copy:—

"*Memorial to the President and Council of The Institution of Civil Engineers.*

"GENTLEMEN,

"Several pupils of Members of your Institution heard and read with great interest the very important and pregnant suggestions made by the President, in the course of his inaugural address, on the subject of the education and training requisite to enable the rising generation of Civil Engineers worthily to maintain the reputation of their predecessors, in the face of the strenuous and increasing competition of their rivals, on the Continent and in America. They feel it to be essential to success in this competition, that they should seize and utilize to the utmost all opportunities now existing for acquirement of, and improvement in, professional knowledge; and, moreover, they submit with deference, that the increasing importance of this matter, in view of the improved systems of special education in other countries, renders it expedient and even necessary that new and better opportunities should be created.

"It has, therefore, occurred to the undersigned Engineering pupils and assistant Engineers, that one means of advancing this important object might be the establishment, under the auspices of your Institution, of what might be called a 'Junior Engineering Society,' to consist of pupils and past pupils of Civil and Mechanical Engineers, with the avowed purpose of mutual self-improvement in professional knowledge among its Members, and more particularly in that scientific knowledge of theory, which is becoming more and more essential to the success of the young Engineer. The means by which they propose to advance this object, are the reading and discussion of Papers by the Members of the Society, and the delivery of Lectures to them on the more recondite branches of Engineering science, by such gentlemen among the Members and Associates of your Institution, and such of their own body, as possessing the requisite knowledge and the power and disposition to impart it, may be induced to confer so great a boon upon those who hereafter will have to maintain the honour and reputation of your great Institution and profession.

"Supposing such a Society to be successfully established, the chief benefit to its Members would be the improvement among them of the feeling of professional *esprit de corps*, and in the establishment and maintenance of a powerful emulation, inciting them to make more and better use of the opportunities afforded them by their position as pupils and assistant Engineers, and by the improvement now going on in professional and technical literature.

"Under the influence of the foregoing consideration, steps have been taken to ascertain whether the projected Society would find the requisite support among the class of persons likely to be benefited by its operations, with what success you will infer from the number of signatures attached to this Memorial. Meetings were held, and a Provisional Committee and Secretary pro tem. appointed, who received instructions to draw a set of Rules to be afterwards ratified by a meeting of the general body. A copy of these Rules is submitted herewith for your approval.

"It is suggested that the proposed Society, to be of real utility in forwarding the object before described, must have a large number of Members, and should include within its ranks at least a majority of the articled pupils, of those Members and Associates of your Institution who practise in London. To this end the public sanction and patronage of your Institution is respectfully solicited, and if you think such a request not too much to ask, the use of your Library, and of your Theatre for the Meetings of the proposed Society, under such restrictions as you may think fit, would be deemed an incalculable boon; and the favour and prestige thereby conferred upon the Society would, as it ought to do, stimulate its Members to make the greatest exertions to render it beneficial to the cause of professional education.

"Should the project now submitted fail to meet your approval, the undersigned will most willingly concur in any measure which you, in your better judgment, may decide to be well adapted to secure the object in view.

"Hoping that this matter will be taken into your favourable consideration at an early date, the undersigned

"Remain,

"Your faithful and obedient Servants,

"E. NOEL EDDOWES, and others."

To this document ninety-three signatures were attached, of Pupils and Assistants engaged in forty different offices, and of that number, seventy-eight were in the offices of thirty-three Members or Associates of the Institution, the remaining fifteen being in seven offices, the principals of which do not belong to the Institution. It will be observed that the Memorial seems mainly to point to the establishment of a subsidiary but self-governed society. The Council informed a deputation of the memorialists, that they would endeavour to devise a plan, for the consideration of the Members generally, which should substantially meet the wishes of the memorialists as far as practicable, without separate organisation; but that they could not support the proposal of independent government. This was at once relinquished by the deputation.

The Council consider that the object of the memorialists may be accomplished by the reconstitution, under certain modifications, of the class of Graduates, or by the establishment of a new class of Students in lieu thereof. Supposing that you concur in this view, then, in either alternative, a revision of the Bye-laws will be necessary, and it will be for the new Council to determine, preparatory to calling a Special General Meeting of Members, requisite for the repeal of existing and the enactment of new Bye-laws, what alterations can be adopted, with due regard to the interests of the Institution as it is now constituted. or as its constitution may be prudently modified by such alterations of the existing Bye-laws. The present Council are decidedly of opinion, that admission to this junior class should be strictly confined to those who are, *boná fide,* in the course of preparation or training for following the profession of an Engineer; and that the members of this class should have no voice in the management of the affairs or in the government of the Institution. The terms and mode of admission to this class, and the privileges to be accorded to its members, are matters which will require careful consideration.

The members should be reminded that, by the will of the late Mr. Joseph Miller, M. Inst. C.E., a sum of £3,000 was bequeathed to the Institution of Civil Engineers "for the purpose of forming a fund (which I desire may be called the 'Miller Fund') for providing premiums or prizes for the Students of the said Institution, upon the principle of the Telford Fund. And I desire that the said sum of £3,000 be invested, and the dividends and interest thereof applied accordingly." The Council, for the time being, having accepted the bequest and undertaken the trust, it was deemed advisable to obtain counsel's opinion as to the construction to be put upon the will. A case was accordingly prepared by the Solicitor to the Institution, and was submitted to Sir Roundell Palmer, Q.C., then Her Majesty's Solicitor-General, whose opinion was given in the following terms:—

"It would, I think, be competent to the Institution to define, by a Bye-law, the class of Students who should be eligible to these prizes. But, in the absence of such a Bye-law, it does not seem to be difficult to place a definite interpretation upon the words of the will. I think it clear that 'all the members generally are not eligible,' nor even 'Graduates,' unless they be Students. Those 'Graduates' who (in the words of the rule as to 'members') are still under 'education' as Civil Engineers, 'according to the usual routine of pupilage,' seem to me to be the only persons who answer properly to the description of 'Students of the Institution.'"

The Council had much pleasure in complying with a request made by the Lords of the Committee of Council on Education, to allot to the intending exhibitors of machinery in the British section of the Paris Universal Exhibition of 1867, the space set apart by the Imperial Commission for that purpose. On the receipt of the detailed lists and applications, it was found that the actual net area available for division amongst the exhibitors was only about one-tenth of that asked for. Communications were therefore opened with many of the firms and individuals who desired to be represented in Paris next year, with a view to ascertain how it was proposed to occupy the area asked for, so as, with fuller and more complete information, to enable the demands to be reduced within the assigned limits. After obtaining these particulars, accompanied in some cases with plans and details, the Council proceeded to revise the lists, and arrived at the best result in their power, in allotting the very limited space at their disposal, in proportion to that demanded. The Council much regretted that, from these unavoidable circumstances, they were unable to accede to a large number of the applications which had been made, and were obliged greatly to limit the space assigned in other cases. The Council received most valuable assistance from Mr. D. K. Clark, M. Inst. C.E., in carrying out this task, and they specially brought his services under the notice of Her Majesty's Commissioners.

From subsequent information it appears, that arrangements have been made for the exhibition of certain objects in the Park surrounding the building, by which a little additional space inside the main building has become available for some of those exhibitors of machinery to whom allotments could not previously be granted.

The additions to the Library, by presentation and by purchase, have been more numerous than usual, as will be seen by the list appended to this Report. The completion of the new Catalogue, an octavo volume of 412 pages, which was issued during the session, will serve to indicate what deficiencies still remain; and the Council would again urge upon every member, the importance of making the collection as complete as possible in all branches of professional literature. During the year there have also been received a bust, by Weekes, of the late Mr. Joshua Field, Past-President Inst. C.E., for which you are indebted to his widow; a portrait, by Collins, of Mr. Hawkshaw, Past-President Inst. C.E.; a portrait, by S. B. Halle, of Mr. Brassey, Assoc. Inst. C.E., which was bequeathed to the Institution by our late Associate, Mr. H. P. Burt; and you will also have noticed the portrait of the late Mr. R. Stephenson, M.P., Past-President Inst. C.E., which has been painted for the Institution by Mr. H. W. Phillips.

The tabular statement of the transfers, elections, deceases, and resignations of members of all classes, during the years 1864-65, and 1865-66 (taking into consideration the names which have been erased from the Register), is as follows:—

YEAR.	Honorary Members.	Members.	Associates.	Graduates.	
1864–65.					
Transferred to Members . .	. .	. .	6	. .	
Elections	3	40	99	. .	142 − 34 = 108
Deceases	1	9	11	. .	34
Resignations	. .	2	3	. .	
Erased from Register . . .	. .	. .	8	. .	
1865–66.					
Transferred to Members . .	. .	. .	12	. .	
Elections	1	53	109	. .	163 − 27 = 136
Deceases	1	11	12	. .	27
Resignations	. .	. .	3	. .	
Members of all Classes on the Books, November 30, 1866 .	20	541	771	7	1,339

In order to show the progress of the Institution during the latter half of its existence, extending over a period of twenty-four years and a half, or from June 30th, 1842, to November 30th, 1866, the following Table has been drawn up :—

DATE.	Honorary Members.	Members.	Associates.	Graduates.	Total of all Classes.	Increase.	
						Actual.	PerCent.
June 30, 1842 .	35	181	241	68	525		
,, 1843 .	35	184	276	65	560	35	6·67
Dec. 31, 1844 .	35	177	285	55	552	−8	−1·43
,, 1845 .	35	194	302	51	582	30	5·43
,, 1846 .	35	207	314	44	600	18	3·09
,, 1847 .	35	214	321	40	610	10	1·67
,, 1848 .	35	224	332	35	626	16	2·62
Nov. 30, 1849 .	36	245	351	32	664	38	6·07
,, 1850 .	34	244	371	32	681	17	2·56
,, 1851 .	33	247	406	30	716	35	5·14
,, 1852 .	30	251	438	26	745	29	4·05
,, 1853 .	30	259	441	20	750	5	0·67
,, 1854 .	28	277	449	19	773	23	3·07
,, 1855 .	27	286	458	16	787	14	1·81
,, 1856 .	26	289	466	16	797	10	1·27
,, 1857 .	26	306	489	14	835	38	4·76
,, 1858 .	25	320	498	14	857	22	2·63
,, 1859 .	25	332	523	14	894	37	4·32
,, 1860 .	24	355	537	14	930	36	4·03
,, 1861 .	22	369	542	12	945	15	1·61
,, 1862 .	20	405	565	10	1,000	55	5·84
,, 1863 .	18	426	587	9	1,040	40	4·00
,, 1864 .	18	452	617	8	1,095	55	5·29
,, 1865 .	20	487	688	8	1,203	108	9·86
,, 1866 .	20	541	771	7	1,339	136	11·30

From this Table it appears that at the former date the total number of members of all classes was 525, as against 1,339 at the 30th of November last, showing an actual increase of 814, which is equal to 155·05 per cent. The annual increase per cent. during the past six years has been 1·61, 5·84, 4·00, 5·29, 9·86, and 11·30 respectively.

The following Table has been compiled in order to show the proportionate number of members of all classes, at the dates referred to, the total number at each period being represented by unity:—

Date.	Honorary Members.	Members.	Associates.	Graduates.
June 30, 1842 . .	·066	·345	·459	·129
,, 1843 . .	·062	·329	·493	·116
Dec. 31, 1844 . .	·063	·321	·516	·100
,, 1845 . .	·060	·333	·519	.088
,, 1846 . .	·058	·345	·523	·074
,, 1847 . .	·057	·351	·526	·066
,, 1848 . .	·056	·358	·530	·056
Nov. 30, 1849 . .	·054	·369	·529	·048
,, 1850 . .	·050	·358	·545	·047
,, 1851 . .	·046	·345	·567	·042
,, 1852 . .	·040	·337	·588	·035
,, 1853 . .	·040	·345	·588	·027
,, 1854 . .	·036	·358	·581	·025
,, 1855 . .	·034	·364	·582	·020
,, 1856 . .	·033	·363	·585	·020
,, 1857 . .	·031	·366	·586	·017
,, 1858 . .	·030	·373	·581	·016
,, 1859 . .	·028	·371	·585	·016
,, 1860 . .	·026	·382	·577	·015
,, 1861 . .	·023	·390	·574	·013
,, 1862 . .	·020	·405	·565	·010
,, 1863 . .	·017	·410	·564	·009
,, 1864 . .	·017	·413	·563	·007
,, 1865 . .	·017	·405	·572	·007
,, 1866 . .	·015	·404	·576	·005

It will thus be seen that the Honorary Members have been relatively diminished from 6·6 to 1·5 per cent., and the Graduates from 12·9 to only 0·5 per cent.; while the number of Members has been increased from 34·5 to 40·4 per cent., and of Associates from 45·9 to 57·6 per cent. Or, in other words, taking the Honorary Members and the Graduates together, at the first date referred to they amounted to nearly one-fifth of the whole number of members, whereas at the present time they only constitute the one-fiftieth part of that number. Nearly one-half of the Members and Associates are resident in London and its vicinity, one-third in other parts of the United Kingdom, and the remainder in British possessions abroad, and in foreign countries.

The following Associates, having tendered their resignation, have been permitted to withdraw from the Institution: Tom Abercrombie Hedley, Jasper Wilson Johns, and George Plucknett.

The deceases announced during the year have been:—The Rev. William Whewell, D.D., Honorary Member; Benjamin Hall Blyth, Robert Daglish, John Dinnen, Modeste Gallez, Simon Goodrich, William Gravatt, General Sir Harry David Jones, Charles Marie Adolphe Nepveu, George Rennie, Alan Stevenson, and Nicholas Wood, Members; John Ashbury, Henry Potter Burt, David Cogan, William Robson Coulthard, James Forbes, Captain Francis Fowke, R.E., William Fisher Hobbs, Arthur James, Edmund Pemell, William Stubbs, Charles Wye Williams, and Colonel Paul Wynch Willis, Associates.

Biographical notices of some of those whose loss the Institution has thus to deplore will be found in an Appendix to this Volume.

The deaths during the twelve months have been at the rate of nearly 18 in the thousand on the present number of members of all classes. It may also be interesting to note, that the name of Dr. Whewell, Honorary Member, had been borne on the books for twenty-nine years; that the eleven deceased Members had belonged to the Institution for periods varying from forty years to six years, the average being upwards of twenty-six years; while of the twelve deceased Associates one had been elected thirty-one years, and another only last year, the average of the whole number being twelve years and a half.

An analysis of the statement of Receipts and Expenditure for the year ending the 30th of November, 1866, as certified by the Auditors, after examining and comparing the Vouchers and the Cash-book, shows that there has been received—

	£.	s.	d.	£.	s.	d.
From subscriptions, fees, &c., exclusive of Building Fund	4,437	14	4			
,, dividends on investments not in trust	506	15	4			
				4,944	9	8
,, Building Fund fees, on election	874	13	0			
,, dividends on investments on this account	119	16	2			
				994	9	2
,, dividends on Trust Funds				360	5	5
Total				£6,299	4	3

The Expenditure during the same period has been—

	£.	s.	d.	£.	s.	d.
To disbursements, including Library Catalogue, Minutes of Proceedings (less received under that head), &c.				4,153	2	4
„ premiums under trusts				130	18	4
„ investments:—						
Telford Fund (unexpended dividends)	509	6	7			
Miller Fund, ditto	525	11	10			
General account	1,510	14	4			
Building Fund	425	5	0			
				2,970	17	9
Total				£7,254	18	5

	£	s.	d.
The balance in the hands of the Treasurer is less than it was at the same date last year by	£953	5	1
And that due to the Secretary on account of petty cash is more than what it then was by	2	9	1
Together	£955	14	2

making up the excess of expenditure over receipts as shown by the previous summary.

The unexpended dividends on the Telford and the Miller Trusts have been invested in Three per Cent. Reduced Consols, and the other sums in Four per Cent. Debenture Stocks of the Great Northern, the North Eastern, and the London and North Western Railway Companies.

There is due to the printer and to the lithographer, for the arrears of publications issued this year, a sum of nearly £2,000. Arrangements will be made to pay off this amount during the Session 1866-67, as well as to meet the current charges, so that the accounts for the financial year 1867-68 may comprise only those proper to the year. Had these liabilities been incurred earlier in the last Session, this desirable result might have been attained twelve months earlier, instead of making the investments before named.

The Funds of the Institution now consist of—

I. General Funds.

		Nominal Value. £.	s.	d.
1. Stephenson Bequest, North Eastern Railway Company's Four per Cent. Debenture Stock		2,000	0	0
2. Miller Bequest, Reduced Three per Cent. Consols		2,133	6	8
3. Errington Bequest, Great Northern Railway Company's Four per Cent. Debenture Stock		1,000	0	0
4. Institution Investments, Great Eastern Railway Company's Four per Cent. Debenture Stock	£3,650			
Carried forward	£3,650	5,133	6	8

	£.	s.	d.	£.	s.	d.	£.	s.	d.
Brought forward	£3,650			5,133	6	8			
Institution Investments. Great Northern Railway Company's Four per Cent. Debenture Stock	1,400								
„ „ London and North Western ditto	1,162								
„ „ London, Brighton, and South Coast ditto	1,000								
				7,212	0	0			
5. Cash in the hands of the Treasurer	£294	16	10						
Less Balance of Petty Cash due to the Secretary	23	14	2						
				271	2	8			
							12.616	9	4

II. BUILDING FUND.

	£.	s.	d.	£.	s.	d.
1. Great Northern Railway Four per Cent. Debenture Stock	500	0	0			
2. London and North Western ditto	1,338	0	0			
3. Great Eastern ditto	1,100	0	0			
				2,938	0	0

III. TRUST FUNDS.

	£.	s.	d.	£.	s.	d.	£.	s.	d.
1. Telford Fund :—									
Three per Cent. Consols	2,551	15	10						
Three per Cent. Annuities	2,342	17	1						
				4,894	12	11			
Unexpended Dividends. Three per Cent. Consols	1,775	19	8						
Ditto. Three per Cent. Annuities	601	10	9						
				2,377	10	5			
2. Manby Donation :—									
Great Eastern Railway Five per Cent. Preference Stock				200	0	0			
3. Miller Fund :—									
Lancashire and Yorkshire Railway Four per Cent. Debenture Stock	2,000	0	0						
Great Eastern Railway, ditto	1,100	0	0						
				3,100	0	0			
Unexpended Dividends. Three per Cent. Consols				582	18	6			
							11,155	1	10
Together amounting to							£26,709	11	2

as against £24,983. 1*s.* 6*d.* at the date of the last Report.

The amount of subscriptions, still remaining due, on the 30th of November, from members of all classes (exclusive of those who are more than three years in arrear, including the current year), is :—

		£.	s.	d.	£.	s.	d.
For 1866.	From members of all classes residing abroad	41	9	6			
	Ditto, in the United Kingdom	236	15	6			
					278	5	0
For 1865.	From members of all classes residing abroad	22	7	6			
	Ditto, in the United Kingdom	84	0	0			
					106	7	6
For 1864.	From members of all classes residing abroad	2	12	6			
	Ditto, in the United Kingdom	40	8	6			
					43	1	0
	Total				£427	13	6

In the preparation of the Balloting List, the regular practice has been followed; and in accordance with the Bye-laws, it will be found to contain the names of seventeen Members and of four Associates. In compliance with a Resolution passed at the last Annual General Meeting, the attendances of the Members of Council in Council and at the Ordinary General Meetings, during the Session 1865-66, have been prefixed to the names of those Members who served on the Council during the last twelve months.

Impressed with the responsibility of the task confided to them at the last Annual General Meeting, the Council have willingly devoted much time and attention to the conduct of the affairs of the Institution. With increased numbers and extended influence, so does it become more and more essential that the Institution, representing a profession the members of which are ever advancing, should not rest satisfied with what has already been accomplished, but that all should zealously endeavour to carry out the work so well commenced by the founders. The Council venture to think their Report will prove, that the progress of the Institution during the past session has been in every respect satisfactory.

ABSTRACT *of* RECEIPTS *and* EXPENDITURE.

ABSTRACT *of* RECEIPTS *and* EXPENDITURE

RECEIPTS.

Dr.	£.	s.	d.	£.	s.	d.
To Balance in the hands of the Treasurer				1,248	1	11
— Subscriptions and Fees:—						
Arrears	145	13	11			
Current	3,446	7	5			
Subscriptions for 1867 (in advance)	14	14	0			
Fees	507	3	0			
Life Compositions	119	10	0	4,233	8	4
— Building Fund				874	13	0
— Publication Fund				129	3	0
— Council Fund				57	10	0
— Publications:—Sale of Transactions				368	1	8
— Telford Fund:—						
Dividends—1 Year, on £2,551. 15*s.* 10*d.*, Three per Cent. Reduced Consols	75	5	3			
Ditto, 1 Year, on £2,342. 17*s.* 1*d.*, Three per Cent. Reduced Annuities	69	2	4			
Ditto, 1 Year, on £1,775. 19*s.* 8*d.*, Three per Cent. Reduced Annuities	52	7	8			
Ditto, ½ Year, on £601. 10*s.* 9*d.*, Three per Cent. Reduced Consols	8	18	9			
Deposit Interest	7	10	2	213	4	2
— Telford Premiums:—						
Repayment of Cost of Binding, &c.				3	13	9
— Manby Premium:—						
Dividends, ½ Year, on £200, Great Eastern Railway Company, Norfolk, Five per Cent. Preference Stock				4	18	4
— Stephenson Bequest:—						
Dividends, 1 Year, on £2,000 North Eastern Railway Company, Four per Cent. Debenture Stock				78	13	4
— Miller Fund:—						
Dividends, 1 Year, on £2,000, Lancashire and Yorkshire Railway Company, Four per Cent. Debenture Stock	78	13	4			
Ditto, 1 Year, on £1,100 Great Eastern Railway Company, Four per Cent. Debenture Stock	43	5	4			
Ditto, ½ Year, on £582. 18*s.* 6*d.*, Three per Cent. Reduced Consols	8	11	2			
Deposit Interest	7	19	4	138	9	2
— Miller Bequest:—						
Dividends, 1 Year, on £2,133. 6*s.* 8*d.*, Three per Cent., Reduced Annuities				62	18	8
— Errington Bequest:—						
Dividends, 1 Year, on £1,000, Great Northern Railway Company, Four per Cent. Debenture Stock				39	6	8
— Institution Investments:—						
Dividends, 1 Year, on £3,650, Great Eastern Railway Company, Four per Cent. Debenture Stock	143	11	2			
Ditto, 1 Year. on £500, Great Northern Railway Company, Four per Cent. Debenture Stock	19	13	4			
Ditto, ½ Year, on £900, ditto ditto	17	14	0			
Carried forward	180	18	6	7,452	2	0

from the 1st DEC., 1865, *to the* 30th NOV., 1866.

PAYMENTS.

Cr.	£.	*s.*	*d.*	£.	*s.*	*d.*
By Balance of Petty Cash due to the Secretary				21	5	1
— House, Great George Street:—						
Repairs	119	11	0			
Rent	368	15	0			
Rates and Taxes	40	0	8			
Insurance	15	18	9			
				544	5	5
— Salaries				750	0	0
— Clerks, Messengers, and Housekeeper				298	7	0
— Postage and Parcels:—						
Postage	34	8	8			
Parcels	0	9	4			
				34	18	0
— Stationery, Engraving, Printing Cards, Circulars, &c.				73	0	8
— Coals, Candles, Oil, and Gas:—						
Coals	13	1	6			
Candles	0	2	3			
Oil	2	5	4			
Gas	33	18	6			
				49	7	7
— Tea and Coffee				32	17	10
— Library:—						
Catalogue	300	19	6			
Books	101	15	5			
Periodicals	19	0	9			
Binding Books	41	5	9			
Evening Attendance	51	5	0			
				514	6	5
— Publication, Minutes of Proceedings				1,827	3	0
— Telford Premiums				118	16	5
— Manby Premium				12	1	11
— Diplomas				33	6	6
— Manuscripts, Original Papers, and Drawings				0	12	2
— Annual Dinner				122	5	2
— Winding and Repairing Clocks				1	10	0
— Portrait of the late Mr. Robert Stephenson, M.P.				116	11	0
— New Building Committee				48	17	1
— Agra and Masterman's Bank. [A dividend of 5*s.* in the Pound has been paid, of which one moiety has been carried to Arrears, and the other to Current Subscriptions.]				6	7	6
— Incidental Expenses:—						
Christmas Gifts	2	0	0			
Assistance at Meetings	9	10	0			
Beating Carpets and Sweeping Chimneys	2	17	0			
Household Utensils, Repairs, and Expenses	49	1	9			
				63	8	9
— Building Fund				425	5	0
— Benevolent Fund				25	17	9
— Paris Exhibition Committee				3	19	11
Carried forward				5,124	10	2

ABSTRACT *of* RECEIPTS *and* EXPENDITURE

RECEIPTS.

Dr.	£.	*s.*	*d.*	£.	*s.*	*d.*
Brought forward	180	18	6	7,452	2	0
To Institution Investments:—						
Dividends, 1 Year, on £1,000, London and North Western Railway Company, Four per Cent. Debenture Stock	39	6	8			
Ditto, ½ Year, on £162, ditto ditto	3	3	10			
Ditto, 1 Year, on £1,000, London, Brighton, and South Coast Railway Company, Four per Cent. Debenture Stock	39	6	8			
Ditto, ½ Year, on £500, North Eastern Railway Company, Four per Cent. Debenture Stock	9	16	8			
Deposit Interest	53	4	4			
				325	16	8
— Special Donations				17	13	0
— Benevolent Fund				25	17	9
— Building Fund Investments:—						
Dividends, 1 Year, on £500, Great Northern Railway, Four per Cent. Debenture Stock	19	13	4			
Ditto, 1 Year, on £500, London and North Western Railway Company, Four per Cent. Debenture Stock	19	13	4			
Ditto, ½ Year, on £838, ditto ditto	16	9	6			
Ditto, 1 Year, on £1,100, Great Eastern Railway Company, Four per Cent. Debenture Stock	43	5	4			
Deposit Interest	20	14	8			
				119	16	2
— Cash withdrawn from deposit				989	5	8
— Balance due to the Secretary				23	14	2
				£8,954	5	5

from the 1st DEC., 1865, *to the* 30th NOV., 1866.

PAYMENTS.

Cr.	£	s.	d.	£	s.	d.
Brought forward				5,124	10	2
By Telford Fund.—Balance of Dividends not yet Expended in Annual Premiums	509	6	7			
— Miller Fund, ditto	525	11	10			
— Institution Investments:—						
— Great Northern Railway Company, £900, Four per Cent. Debenture Stock	872	19	0			
— North Eastern Railway Company, £500, Four per Cent. Debenture Stock	484	19	3			
— London and North Western Railway Company, £162, Four per Cent. Debenture Stock	152	16	1			
				2,545	12	9
— On deposit				989	5	8
				8,659	8	7
— Balance, Dec. 1, 1866, in the hands of the Treasurer				294	16	10
				£8,954	5	5

Examined and compared the above Account with the Vouchers and the Cash Book, and find this account to be correct, leaving a Balance in the hands of the Treasurer of Two Hundred and Ninety-four Pounds, Sixteen Shillings, and Ten Pence.—Nov. 30th, 1866.

(Signed) F. W. SHEILDS, GEORGE P. BIDDER, Jun., } *Auditors.*
JAMES FORREST, *Secretary.*

December 7th, 1866.

PREMIUMS AWARDED.

Session, 1865-66.

The Council of the Institution of Civil Engineers have awarded the following Premiums:—

1. A Telford Medal, and a Telford Premium, in Books, to Richard Price Williams, M. Inst. C.E., for his Paper "On the Maintenance and Renewal of Permanent Way."
2. A Telford Medal, and a Telford Premium, in Books, to John Grant, M. Inst. C.E., for his Paper "Experiments on the Strength of Cement, chiefly in reference to the Portland Cement used in the Southern Main Drainage Works."
3. A Telford Medal, and a Telford Premium, in Books, to Edwin Clark, M. Inst. C.E., for his Paper on "The Hydraulic Lift Graving Dock."
4. A Telford Medal to Sir Charles Tilston Bright, M.P., M. Inst. C.E., for his Paper on "The Telegraph to India, and its Extension to Australia and China."
5. A Telford Medal, and the Manby Premium, in Books, to Robert Manning, M. Inst. C.E., for his Paper "On the Results of a Series of Observations on the Flow of Water off the Ground in the Woodburn District, near Carrickfergus, Ireland; with Rain-gauge Registries in the same locality, for a period of twelve months, ending 30th June, 1865."
6. A Telford Premium, in Books, to William Humber, Assoc. Inst. C.E., for his Paper "On the Design and Arrangement of Railway Stations, Repairing Shops, Engine Sheds, &c."
7. A Telford Premium, in Books, to George Rowdon Burnell, M. Inst. C.E., for his Paper "On the Water Supply of the City of Paris."
8. A Telford Premium, in Books, to William Ridley, Assoc. Inst. C.E., for his Paper on "The Grand River Viaduct, Mauritius Railways."
9. A Telford Premium, in Books, to Theodore Anthony Rochussen, Assoc. Inst. C.E., for his Paper "On the Maintenance of the Rolling Stock on the Cologne-Minden, and other Prussian Railways."
10. A Telford Premium, in Books, to William Hemingway Mills, M. Inst. C.E., for his Paper on "The Craigellachie Viaduct."

SUBJECTS FOR PREMIUMS.

Session, 1866-67.

The Council of the Institution of Civil Engineers invite communications on the Subjects comprised in the following list, as well as upon others; such as—1. Authentic Details of the Progress of any Work in Civil Engineering, as far as absolutely executed (Smeaton's Account of the Edystone Lighthouse may be taken as an example); 2. Descriptions of Engines and Machines of various kinds; or, 3. Practical Essays on Subjects connected with Engineering, as, for instance, Metallurgy. For approved Original Communications, the Council will be prepared to award the Premiums arising out of special Funds devoted for the purpose.

The Council will be glad to receive, for the purpose of forming an "Appendix" to the Minutes of Proceedings, the details and results of any Experiments, or Observations, on Subjects connected with Engineering Science, or Practice.

1. On the Theory and Details of Construction of Metal and Timber Arches.
2. On Land-slips, with the best means of preventing, or arresting them, with examples.
3. On the Principles to be observed in Laying-out lines of Railway through mountainous countries, with examples of their application in the Alps, the Pyrenees, the Indian Ghâts, the Rocky Mountains of America, and similar cases.
4. On Railway Ferries, or the Transmission of Railway Trains entire across Rivers, Estuaries, &c.
5. On the Pneumatic System for the conveyance of Passengers and Goods.
6. On the Systems of Fixed Signals at present in use on Railways.
7. On Light Railways in India, Norway, Queensland, and other places.
8. On the most suitable Materials for, and the best mode of formation of, the Surfaces of the Streets of large Towns.

9. On the Construction of Catch-water Reservoirs in Mountain Districts, for the Supply of Towns, or for Irrigation, or Manufacturing Purposes.
10. Accounts of existing Waterworks; including the source of supply, a description of the different modes of collecting and filtering, the distribution throughout the streets of Towns, and the general practical results.
11. On the Benefits and Expedients of Irrigation in India and in other warm climates; and on the proper Construction of Irrigating Canals, so as to avoid erosion or silting, and to prevent the growth of weeds.
12. On the best mode of Deodorising and Filtering, or otherwise of precipitating Sewage, and of applying it to the land.
13. On the Ventilation of Sewers.
14. On the Ventilation and Warming of Public Buildings.
15. On the best means of Manufacturing Gas of high Illuminating Power; and on the Construction of Gasworks, the most economical system of distribution of Gas, and the best modes of Illumination in Streets and Buildings.
16. A History of any Fresh Water Channel, Tidal River, or Estuary,—accompanied by plans and longitudinal and cross sections of the same, at various periods, showing the alterations in its condition,—including notices of any works which may have been executed upon it, and of the effects of the works; particularly of the relative value of Tidal and Fresh Water, of the effect of Enclosures from the Tidal Area upon the general régime, of Sluicing where applied to the improvement of the entrance or the removal of a Bar, and of Groynes, or Parallel Training Walls. Also, of Dredging, with a description of the Machinery employed, and the cost of raising and depositing the material.
17. On the Construction of Tidal, or other Dams, in a constant, or variable depth of water; and on the use of Wrought Iron in their construction.
18. On the Arrangement and Construction of Floating Landing-Stages, for passenger and other traffic, with existing examples.
19. On the different systems of Swing, Lifting, and other opening Bridges, with existing examples.
20. On the Construction of Lighthouses, their Machinery and Lighting Apparatus; with notices of the methods in use for distinguishing the different Lights.
21. On the Measure of Resistance to Bodies passing through Water at high Velocities.
22. On Ships of War, with regard to their Armour, Ordnance, mode of Propulsion, and Machinery.

23. On the measures to be adopted for protecting Iron Ships from Corrosion.
24. On the Results of the Employment of Steam Power on Canals, and of other measures for the Improvement of Canals as a means of conveyance for heavy traffic.
25. On the Construction and Performance of Turbines of all classes.
26. On the comparative cost of Conveying Coals by Railways and by Screw Colliers.
27. On the present systems of Smelting Iron Ores; of the conversion of Cast Iron into the malleable state, and of the manufacture of iron generally, comprising the distribution and management of Ironworks.
28. On the Manufacture of Iron for Rails and Wheel Tires, having special reference to the increased capability of resisting lamination and abrasion; and accounts of the Machinery required for rolling heavy Rails, Shafts, and bars of Iron of large sectional area.
29. On the Bessemer and other processes of Steel-making; on the present state of the Steel Manufacture on the Continent of Europe; and on the employment of castings in Steel for Railway Wheels and other objects.
30. On the Use of Steel for the Tires and Cranked Axles of Locomotive Engines, especially with reference to its durability and the cost of repairs, as compared with Iron of acknowledged good quality; and on the use of Steel Bars and Plates generally in Engine-work and Machinery, for Boilers and for Shipbuilding, as well as for Bridges.
31. On the safe working strength of Iron and Steel, including the results of experiments on the Elastic Limit of long bars of Iron, and on the rate of decay by rusting, &c., and under prolonged strains.
32. On the present state of Submarine Telegraphy, and on the Transmission of Electrical Signals through Submarine Cables.
33. On the present relative position of English and Continental Engineering Manufactories, especially with reference to their comparative positions in respect of the cost, and the character of the work produced.

The Council will not consider themselves bound to award any Premium, should the Communication not be of adequate merit, but they will award more than one Premium, should there be several communications on the same subject deserving this mark of distinction. It is to be understood that, in awarding the Premiums, no distinction is made, whether the Communication has been received from a Member, or an Associate of the Institution, or from any other person, whether a Native, or a Foreigner.

The Communications must be forwarded, on or before the 1st of February, 1867, to the house of the Institution, No. 25, Great George Street, Westminster, S.W., where copies of this Paper, and any further information, may be obtained.

CHARLES MANBY, *Honorary Secretary.*
JAMES FORREST, *Secretary.*

25, *Great George Street, Westminster, S.W.,*
October, 1866.

NOTICE.

It has frequently occurred, that in Papers which have been considered deserving of being read and published, and have even had Premiums awarded to them, the Authors may have advanced somewhat doubtful theories, or may have arrived at conclusions at variance with received opinions. The Council would, therefore, emphatically repeat, that the Institution must not, as a body, be considered responsible for the facts and opinions advanced in the Papers, or in the consequent Discussions; and it must be understood, that such Papers may have Medals and Premiums awarded to them, on account of the Science, Talent, or Industry displayed in the consideration of the subject, and for the good which may be expected to result from the discussion and the inquiry; but that such notice, or award, must not be considered as any expression of opinion, on the part of the Institution, of the correctness of any of the views entertained by the Authors of the Papers.

EXTRACTS FROM THE MINUTES OF COUNCIL, FEB. 23rd, 1835.

"The principal Subjects for which Premiums will be given are:—

"1st. Descriptions, accompanied by Plans and explanatory Drawings, of any work in Civil Engineering, as far as absolutely executed, and which shall contain authentic details of the progress of the work. (Smeaton's Account of the Edystone Lighthouse may be taken as an example.)

"2ndly. Models, or Drawings, with descriptions of useful Engines and Machines: Plans of Harbours, Bridges, Roads, Rivers, Canals, Mines, &c.; Surveys and Sections of Districts of Country.

"3rdly. Practical Essays on subjects connected with Civil Engineering, such as Geology, Mineralogy, Chemistry, Physics, Mechanic Arts, Statistics, Agriculture, &c.; together with Models, Drawings, or Descriptions of any new and useful Apparatus, or Instruments applicable to the purposes of Engineering, or Surveying."

EXCERPT BYE-LAWS, SECTION XIV., CLAUSE 3.

"Every Paper, Map, Plan, Drawing, or Model presented to the Institution shall be considered the property thereof, unless there shall have been some previous arrangement to the contrary, and the Council may publish the same, in any way and at any time they may think proper. But should the Council refuse, or delay the publication of such Paper, beyond a reasonable time, the Author thereof shall have a right to copy the same, and to publish it as he may think fit, having previously given notice, in writing, to the Secretary of his intention. No person shall publish, or give his consent for the publication of any communication presented and belonging to the Institution, without the previous consent of the Council."

INSTRUCTIONS FOR PREPARING COMMUNICATIONS.

The Communications should be written in the impersonal pronoun, and be legibly transcribed on foolscap paper, about thirteen inches by eight inches, the lines being three-quarters of an inch apart, on the one side only, leaving a margin of one inch and a half in width on the left side, in order that the sheets may be bound.

The Drawings should be on mounted paper, and with as many details as may be necessary to illustrate the subject. Enlarged Diagrams, to such a scale that they may be clearly visible, when suspended on the walls of the Theatre of the Institution, at the time of reading the communication, should be sent for the illustration of any particular portions.

Papers which have been read at the Meetings of other Scientific Societies, or have been published in any form, cannot be read at a Meeting of the Institution, nor be admitted to competition for the Premiums.

ORIGINAL COMMUNICATIONS,

DRAWINGS, PRESENTS, &c.,

RECEIVED BETWEEN JUNE 30th, 1865, AND JUNE 29th, 1866.

ORIGINAL COMMUNICATIONS.

AUTHORS.

Barlow, W. H. No. 1,156.—Description of the Clifton Suspension Bridge.

Bourne, J. No. 1,163.—Ships of War.

Buchanan, W. C. No. 1,159.—Description of the Southern Railway of Chile.

Buckham, T. No. 1,158.—On the Drainage and Water Supply of the Town of Fareham.

Clark, E. No. 1,155.—The Hydraulic Lift Graving Dock. With One Drawing.

Clegram, W. B. No. 1,154.—Results of the Employment of Steam Power in Towing Vessels on the Gloucester and Berkeley Canal. With One Diagram.

Doyne, W. T. No. 1,142.—Description of improved Suspension Gear for correcting the Oscillating and Rocking Motions produced by Girders on the Piers of Long Bridges. With Model.

Folkard, A. No. 1,150.—The Coasting Service of India. With Four Drawings.

Fox, C. D. No. 1,166.—On Light Railways in Norway, India, and Queensland. With a Series of Diagrams.

Gibson, J. W. No. 1,149.—On Opening Bridges constructed on the Telescope, or Sliding Principle, for Railways and Ordinary Roadways crossing Dock Entrances, Canals, &c. With Two Drawings.

Graham, R. W. No. 1,161.—Description of the Bhore and Thul Ghât Inclines on the Great Indian Peninsula Railway. With a Series of Diagrams.

Healy, S. No. 1,165.—On the Employment of Steam Power on the Grand Canal, Ireland. With Models.

Manning, R. No. 1,153.—On the Results of a Series of Observations on the Flow of Water off the Ground, in the Woodburn District, near Carrickfergus, Ireland; with Rain-gauge Registries in the same locality, for a period of twelve months, ending 30th June, 1865. With One Diagram.

Morgan, J. L. No. 1,144.—On the Smelting of Refractory Copper Ores, with Wood as Fuel, in Australia. With One Drawing.

Naylor, W. No. 1,143.—Motive Power and Break Power on Railways.

O'Connell, Lieut.-Col. P. No. 1,167.—On the Relation of the Fresh Water Floods of Rivers to the Areas and Physical Features of their Basins; and on a Method of Classifying Rivers and Streams with reference to the Magnitude of their Foods, proposed as a means of facilitating the investigation of the Laws of Drainage.

Ormsby, A. S. No. 1,140.—On Railway Accidents and their Prevention.

Ridley, T. D. No. 1,148.—Description of the Cofferdams used in the execution of No. 2 Contract of the Thames Embankment. With Five Drawings.

Ridley, W. No. 1,145.—The Grand River Viaduct, Mauritius Railways. With Sketches and enlarged Diagrams.

Rochussen, T. A. No. 1,162.—On the Maintenance of the Rolling Stock on the Cologne-Minden, and other Prussian Railways. With a Series of Diagrams.

Stiffe, A. W. No. 1,168.—On the Repairs to, and present condition of, the Persian Gulf Telegraph Cable.

Turner, W. No. 1,151.—On Moving and Opening Bridges.

Tyler, Capt. H. W. No. 1,160.—On the working of Steep Gradients and Sharp Curves on Railways.

Wheeler, W. H. No. 1,147.—Description of the River Witham and its Estuary, and of the various Works carried out in connection therewith for the Drainage of the Fens and the Improvement of the Navigation. With Two Plans.

Williams, R. Price. No. 1,152.—On the Maintenance and Renewal of Permanent Way. With a Series of Diagrams.

CATALOGUE OF PRESENTS.

BOOKS.

DONORS.	TITLE OF WORK.
Académie	Royale de Belgique. Annuaire. 12mo. Portrait. Bruxelles, 1865.
———	Bulletins de l'Académie. 2^{me} serie. Tome XVIII., 1864. Tome XIX., 1864. 2 vols. 8vo. Plates and Cuts. Bruxelles, 1864-65.
———	Mémoires Couronnés et Autres Mémoires. Publiés par l'Académie. Tome XVII. 8vo. Plate. Bruxelles, 1865.
———	Mémoires Couronnés et Mémoires des Savants Etrangers. 4to. Coloured Plates and Cuts. Bruxelles, 1865.
Adams, W. G.	On the Application of Screw Blades as Floats for Paddle-wheels. By W. G. Adams. Tract. 8vo. No place or date. [Excerpt "Phil. Mag.," for April, 1865.]
———	On the Application of the principle of the Screw to the Floats of Paddle-wheels. Tract. 8vo. Cuts. No place or date. [Excerpt "Phil. Mag.," for May, 1865.]
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Adolph, W.	The Simplicity of the Creation; or, The Astronomical Monument of the Blessed Virgin, a new theory of the Solar System, Thunderstorms, Waterspouts, Aurora Borealis, &c., and the Tides. By William Adolph. 8vo. Cuts. London, 1864.
Airy, G. B.	Address to the Individual Members of the Board of Visitors of the Royal Observatory, Greenwich, Oct. 21, 1865, and Report to the Board of Visitors of the Royal Observatory, Greenwich, read at the Annual Visitation of the Royal Observatory, June 2, 1866. By G. B. Airy, Astronomer Royal. 4to. London, 1866.
———	Essays on the Roman Invasion of Britain: with Correspondence. By G. B. Airy, Astronomer Royal. 4to. Plates. London, 1865. [Printed for private circulation.]

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[From the "Lahore Chronicle."]

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——— Soufflérie Aspirante; remplaçant les Machines Soufflantes et Ventilateurs. Par T. de Beauregard. Tract. 8vo. Plate. Paris, 1865.

——— The Prevention of Panics; or, Suggestions for an Economical System of National Finance in connection with the Construction of Public Works in any country of the World, without either Subscriptions, Loans, Mortgages, Bonds, or Interest. By a Civil Engineer. 8vo. Second Edition. London, 1866.

Anstruther, Major-Gen. The Flight of Projectiles, together with an Instrument projected by him to show the trajectory of a Ball from a given elevation and range, together with its initial velocity and force of striking. By Major-General Anstruther. C.B. Tract. 8vo. London, 1866.

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——— Ditto, Plates. Letters F—T. No. 1, years 1863, 1864, 1865. Folio. London, 1866.

Bache, Professor A. D. Report of the Superintendent of the (U. S.) Coast Survey, showing the Progress of the Survey during the year 1862. 4to. Plates. Washington, 1864.

Balfour, J. M. New Zealand Exhibition, 1865. Results of a Series of Experiments on the Strength of Colonial Woods; being Appendix C to the Jurors' Reports. By James Melville Balfour, C.E, Tract. 8vo. No place or date.

Bateman, J. F. Metropolis Water Supply. On the Supply of Water to London from the Sources of the River Severn. By J. F. Bateman, F.R.S. Tract. 8vo. Maps. London, 1865.

Bazalgette, J. W. Metropolitan Sewers. Concise Statement of the Main Features of the Plans for the Drainage of the Metropolis, sent in pursuance of the Resolution of the Court, 30th Aug. 1849. 2 vols. 8vo. London, 1849.

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[From the "Journal of the Royal United Service Institution," vol. ix.]

Byrne, O., through Captain Fishbourne, R.N. The Young Dual Arithmetician; or, Dual Arithmetic. A New Art designed for elementary instruction and the use of schools. To which are added tables of ascending and descending dual logarithms, dual numbers and corresponding natural numbers. 8vo. London, 1866.

Canadian Institute. The Canadian Journal of Industry, Science, and Art. New Series. Nos. 57-62. May, 1865—April, 1866. 8vo. Toronto, 1865-66.

Cato, C. J. Dottings on Natal, June, 1865. By J. D. Holliday. Tract. 8vo. Maritzburg, 1865.

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body, and entitled, "On the Adhesion of Locomotive Engines, and certain expedients for supplementing that Function." By Zerah Colburn. Tract. 8vo. London, 1865.

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[Lithographed.]

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———— A Map of y^e^ Great Levell of y^e^ Fenns extending into y^e^ counties of Northampton, Norfolk, Suffolke, Lyncolne, Cambridg, and Huntington and the Isle of Ely, as it is now drained. By J. Moore. Four parts folding in 4to. London, no date.

———— Address to the Gentlemen Land-owners in the parts of South Holland, in the county of Lincoln. By Capt. J. Perry. Folio. Maps and cut. London, 1727.

———— An Inquiry into Facts, and Observations thereon. Humbly submitted to the candid examiner into the principles of a bill intended to be offered to Parliament, for the preservation of the Great Level of the Fens, and the navigation through the same, by a tax on the lands, and a toll on the navigation. 8vo. London, 1777.

———— Bedford Level Petition, presented to the House of Commons. With report of the Committee to whom the petition was referred. 8vo. No place, 1777.

———— Complaints about Navigation. By the Merchants, Mariners, and Watermen of Lynn. Folio. No place, 1724.

———— Considerations on the principles of Mr. Rennie's plan for the Drainage of the North Level, South Holland, &c., with a view to their practical adoption. By T. Wing. 8vo. Peterborough, 1820.

———— Eau Brink New River or Cut. Deed Poll, stating the opinion (in the nature of an award) of J. Huddart, in pursuance of the reference to him by Sir Thomas Hyde Page, and Robert Mylne. Under the powers of an Act of Parliament, 35th Geo. III., cap. 77. 4to. Plates. London, 1804.

———— Exaction of Tythes. The Petition of the Inhabitants of the Parish of Elm cum Emneth, in the counties of Norfolk and Cambridge, intended to be presented to the Honorable the House of Commons in the present Session. By S. Wells. 8vo. Cambridge, 1827.

Corporation of the Bedford Level. Extracts from the Report of a View of the Great Level of the Fenns, called Bedford Level; taken in the summer of the year 1777, at the desire of the Board. By C. N. Cole. 8vo. No place, 1784.

———— Joint Report of Messrs. Walker and Mylne, the Engineers appointed in consequence of the late intended Eau Brink Act. 4to. Cambridge, 1825.

———— Letter to his Grace the Duke of Bedford, Governor of the Bedford Level Corporation, on the works in the new Bedford, or 100-feet River, the River Ouze, and other matters of the Corporation. By S. Wells. 8vo. Cambridge, 1828.

———— Letter to the Right Hon. the Earl of Hardwicke, on the use of the under-excavating or steam-dredging engine in deepening the rivers in the Bedford Level; with observations on the effects of the Eau Brink Cut, and on other matters connected with the drainage of the Level. By T. Archer. 8vo. Ely, 1829.

———— North Level of the Fens. An Historical Account of the North Level of the Fens, referring to legal decisions connected with such ancient laws of sewers, contracts, and Acts of Parliament, as the claims are founded upon, with respect to the equal rights of drainage and preservation of the whole North Level, including Porsand, and the uninclosed parts of that Level. 4to. London, 1809.

———— Observations and Schemes for Draining the South Level. By Lieut. Page. 4to. London, 1775.

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———— Observations relative to a bill intended to have been presented to Parliament this sessions, for the better draining the Middle and South Bedford Levels, and the improvement of Navigation. By A. B. 8vo. No place, 1778.

———— Proposed improvements in the drainage of the Middle and South Levels. By S. Wells. 8vo. London, 1842.

———— Remarks on certain Queries delivered in by the Rev. Mr. Dickinson, at a meeting held at Wisbeach, on Wednesday, the 5th of September, 1770. By R. Dunthorne. 4to. No place or date.

———— Report and Estimate on the Improvement of the Drainage and Navigation of the South and Middle Levels of the great level of the Fens. In consequence of a reference from the Honorable Corporation of the Bedford Level, made the 24th May, 1809. By John Rennie. 4to. Plates. London, 1810.

Corporation of the Bedford Level. Reports as to the Wisbeach Outfall, and the Drainage of the North Level and South Holland. By John Rennie. With a Statement, &c., as to the drainage of the Fens north of Boston. By Anthony Bower. 4to. Maps. London, 1814.

———— Report concerning the Drainage of the North Level of the Fens, and the Outfall of the Wisbeach River. By J. Golborne. 4to. Chester, 1769.

———— Report for amending the outfall of the Nene, securing the Middle and North Levels of the Fens, and other countries adjoining to them, from inundations of the North and South Bank of Morton's Leame, and improving the navigation of the Port of Wisbeach, and for better draining the lands above mentioned. 4to. No place. 1777.

———— Report for the better drainage of the South and Middle Levels of the Fens, and the lands bordering upon each side of the River Ouse, and amending the Outfall of the said River, by a new cut or channel from Eau Brink to Lynn. By J. Watté. 4to. Plan and section. Wisbeach, 1791.

———— Report, in pursuance of several resolutions passed at a meeting of the Committee of Landowners, and others, interested in the improvement of the Outfall of the River Ouse, at the Crown and Anchor, in the Strand, on Thursday, the 16th of June, 1791; and read before the same Committee, at the Rose Tavern, in Cambridge, on Wednesday, the 31st of August in the same year; and which is now printed by order of the said Committee. By J. Golborne. 4to. Lynn, 1791.

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———— Report on the Drainage of the Middle Level part of the Great Level of the Fens; called the Bedford Level, and on the state of the River Ouse, below Denver Sluice, in reference to the Drainage of the South Level. By James Walker. 4to. Plan. London, 1842.

———— Report on the 100-feet Wash and Wash Bank. By John Rennie. London, 1812.

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——— Révue Géologique pour les années 1862-63. Par MM. Delesse et Laugel. 8vo. Paris, 1865.

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——— On the Application of Steam Power to Cultivation. By John Fowler and David Greig. Tract. 8vo. Plates. Birmingham, 1865.

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January 8, 1867.

CHARLES HUTTON GREGORY, Vice-President, in the Chair.

The following Candidates were balloted for and duly elected:—John Clark, Lewis Henry Moorsom, James Long Parker, Charles Sacré, and Edwin Thomas, as Members; and Adam Fettiplace Blandy, as an Associate.

The discussion upon the Paper, No. 1,134, "On the best means of communicating between the Passengers, Guards, and Drivers of Trains in Motion," occupied the evening, to the exclusion of any other subject.

January 15, 1867.

CHARLES HUTTON GREGORY, Vice-President,
in the Chair.

No. 1,163.—"Ships of War."[1] By John Bourne.

The Author wishes to invoke the attention of the Institution to a topic which is not merely of interest to Engineers, but is of vital importance to the nation at large—the proper construction of Ships of War.

It is well known that the Admiralty has already constructed two iron-clad fleets, of which the 'Warrior' may be taken as the type of one, and the 'Bellerophon' of the other. Yet not a vessel of either fleet is capable of repelling the shot from ordnance, even of third or fourth-rate dimensions; and another iron-clad fleet is now about to be built, which, unless some extraneous force should interpose, there is every reason to fear will be similarly ineffective. The sides of the 'Warrior' are covered with 4½-inch plates on 18 inches of teak, and the sides of the 'Bellerophon'—the strongest of the existing iron-clads—with 6-inch plates on 10 inches of teak. But a 9-inch gun, charged with 43 lbs. of powder, lately sent a Palliser chilled cast-iron shell, weighing 250 lbs., through an 8-inch plate backed with 18 inches of teak. What chance, then, can any of the iron-clad ships have when assailed with such missiles? It will be obvious to every one, that an armour-clad vessel which is not impenetrable is worse than a vessel without armour at all; as she is not only encumbered with a useless weight, but the iron punched out will be projected into the vessel in a cone of destructive splinters, carrying havoc among the crew. It has, consequently, been suggested, that it might be preferable to revert to the use of wooden ships, rather than to rely upon the defence afforded by armour which, in the day of trial, would prove a mockery and a snare. That this, however, is the only or proper alternative, no competent authority would be disposed to maintain. But, as the problem is one of surpassing importance, and as the proper construction of ships and of armour is, in all its parts, an engineering question, which can be dealt with at the Institution with more intelligence and authority than anywhere else, the Author ventures to bring it forward for consideration.

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[1] The discussion upon this Paper occupied portions of four evenings, but an abstract of the whole is given consecutively.

The Author is of opinion, that vessels built on the Monitor system of Ericsson are the only ones which are capable of carrying sufficient thickness of armour to resist modern ordnance; and any Monitors now to be built, he would propose, should have side armour 18 inches thick backed by 4 feet of oak, and a turret 24 inches thick, carrying two 20-inch wrought-iron guns. Such a vessel could be constructed on a displacement but little different from that of the 'Bellerophon,' and would not only be impenetrable now, but would probably remain so for some years. It is mere fatuity, however, to go on building vessels with such thin armour as 8 inches or 10 inches, which either existing guns can pierce, or guns a little more powerful—and such as will be sure to make their appearance in the next few years—will certainly be able to pierce. Thick armour, however, cannot be applied to any vessels of moderate dimensions unless they are built upon the Monitor system, with a very low side, with a turret to carry the guns, and with effective means for preventing the water which comes upon the deck from entering the vessel. At one time the impression was widely entertained, that the Monitor vessels, from being so low in the water, were not seaworthy, and could not be rendered so. This has been shown by ample experience to be a complete mistake. It is also a mistake to suppose that these vessels do not afford comfortable accommodation for the crew. By statistical returns it appears that they are the most healthful vessels in the American fleet. They are also the most popular with the sailors. Nevertheless, such is the intractability of naval prejudice in this country, that, up to the present moment, while there is an abundance of frail antediluvian arks—powerless for offence and equally powerless for resistance—there is not a single Monitor in the Royal Navy. The fact is, the art of maritime war has entered upon a phase with which naval men, as a rule, are but little qualified to deal. It is a question of a preponderance of forces; and the Monitor is the engineering solution of that problem. The common iron-clad is merely an ordinary ship covered with armour to keep the balls out; and as the armour has to be spread over a high side, it is necessarily thin and weak. In the Monitor system the area to be protected is reduced to a minimum; and, consequently, with the same displacement of vessel the armour may be made thick. In both the armour and the guns the broadside system is a system of diffusion—the Monitor system one of concentration; and in any contest between a 'Monitor' and a broadside vessel of equal size, the broadside vessel must necessarily succumb, simply because a large force, whether of penetration or of resistance, must necessarily prevail over a small one. The Kalamazoo class of Monitors, built by the Americans, has side armour 14 inches thick backed by several feet of oak; and these vessels possess great facility of evolution, as they are fitted with balanced rudders and twin screws.[1]

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[1] The 'Puritan' is armed with Rodman's guns of 20-inch bore; and it is understood that, in America, the construction of guns of 25-inch bore is contemplated. The service charge of powder of the 20-inch Rodman gun is 100 lbs. to 120 lbs. The most powerful gun yet constructed in this country is Sir W. Armstrong's 22-ton gun, of 13¼ inches bore; and, by referring to Captain Inglis' tables showing the action of the shot from this and other guns, on different targets, it will be seen that a cylindrical steel shot weighing 612 lbs., fired from this gun with 70 lbs. of powder, penetrated a 6½-inch armour plate with 18 inches of teak backing, placed on a skin and frames like those of the 'Warrior.' With a Palliser chilled cast-iron shot greater penetration is effected than with a steel shot. A steel shot, when picked up after having been fired against a heavy target, is found to be quite hot: showing that a part of the energy has been transformed into heat. The chilled shot, on the contrary, is found to be cold and unaltered in form; so that, in its case, the whole energy is expended in penetrating the plate. The *vis viva* generated by a pound of powder in the large guns of the Americans is greater than in the smaller guns used in the British service; as might naturally be concluded from the fact, that in the large guns the powder gas works more expansively. A gun is virtually a cylinder: the ball is a piston and the powder answers to the steam; and in a gun, as in a cylinder, a given weight of powder, if used very expansively in a large cylinder, will generate more power than if used less expansively in a small one. The length of the bore of Sir W. Armstrong's 22-ton gun is 145 inches; so that its capacity is 20,010 cubic inches. The length of the bore of Rodman's 20-inch gun is 163 inches; so that its capacity is 51,182 cubic inches. It is quite certain that the *vis viva* of the shot from the 20-inch gun, burning 120 lbs. of powder, will be about twice as great as that of the shot from the 13¼-inch gun, burning 70 lbs. of powder. But if the weaker shot penetrates the existing iron-clads, it is clear they will not be able to resist the stronger, and that no margin is left for the future. Ericsson has constructed a 13-inch gun composed of longitudinal bars welded into a cylinder, which, having been truly turned, is strengthened by washers of plate iron being forced over it by hydraulic pressure; and this gun having been very successful, he now proposes to construct a 20-inch wrought-iron gun on the same principle. It is well known that the difficulty of giving strength to thick cylinders of iron strained from within, is owing to the unequal circumferential lengths of the different layers, which have to be equally extended; and as in the inner layer this extension has to take place through a small circumference, and in the outer layer through a large circumference, the inner layer may be ruptured before much strain has been put upon the outer layer, and the gun will be burst in detail, in the manner in which a sheet of paper is torn. To remedy this defect in the structure of guns, many expedients have been propounded. Guns tightly wound round with silk are used by the Chinese; and nearly twenty years ago the Author examined, in the Museum at Malta, a gun consisting of a copper tube wound round with cord, which had been used by the Knights of St. John against the Turks. Mr. Longridge, in a Paper read before the Institution,[1] has proposed to increase the strength of guns by wrapping them round with wire, the strain upon the different layers being adjusted in the winding by suitable mechanism, so that when the gun is fired every part of its constituent metal will be strained alike. It has been objected to Mr. Longridge's plan, that there is no good way of fixing the end of the wire, which might unravel, especially if the outside of the gun happened to be struck by shot. But nothing would be easier than to solder the whole of the wires together by the electrotype process; and by Mr. Longridge's method of construction there is certainly the means of fabricating more powerful guns than any yet introduced. Some guns are

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[1] *Vide* Minutes of Proceedings of the Inst. C. E., vol. xix., p. 283.

The system of broadside armament is the one which has been adopted in France and in England; and the turret, or Monitor system, has been widely adopted in America. In the broadside

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formed of concentric cylinders of wrought iron shrunk on a steel or homogeneous metal tube; and in some cases the hoops or cylinders, instead of being shrunk on, are forced on by hydrostatic pressure, in the manner employed by Ericsson for his washers. But in each case the object is the same, namely, to place the metal near the bore under compression, and the metal near the circumference under extension, in the ordinary unloaded condition of the gun, to the end that all the metal may be strained equally when the gun is fired. In Rodman's guns, which are made of cast iron, the same end is attained by casting the gun hollow, and by cooling it from the inside while the outside is kept hot by fires. By this arrangement the metal nearest the bore soonest solidifies and contracts to the dimensions proper to a low temperature. The contiguous layer next solidifies; but being still red hot, it contracts upon the layer first cooled, as any other red-hot ring would do; and while it is thus itself extended, it places the first layer under compression. The next layer in like manner compresses the preceding one, and while discharging the tension of the preceding layer it is placed under tension itself; and the general effect of the arrangement is to put the internal parts of the gun under conpression and the external parts under tension, with such a graduation of the strain from the centre to the circumference, that the whole of the constituent metal of the gun will be equally stretched at the moment of bursting. In such a gun the first effect of the powder on the inner layer will be to relieve it from compression; and a considerable charge of powder may be fired without bringing the metal of the inner layer into tension at all. Such guns will not be burst in detail; and it is practically found that Rodman's guns, though constructed of cast iron, which is a weak material, may be used of twice the area of bore and with twice the charge of powder generally employed. There is nothing, however, to prevent this principle of construction from being used with strong materials, such as homogeneous metal and steel; and more powerful guns may be thus constructed than any which have heretofore been introduced.

But besides the source of weakness in large guns here indicated, such guns are subjected to an increased internal pressure, from the increased weight of the shot relatively with the area on which the pressure of the powder acts. The shot of a gun is like the safety-valve of a boiler; and the heavier the shot is made, relatively with the pressed area, the larger will be the internal pressure generated to force it out. If the diameter of the bore of a gun is doubled, the area of the bore will be increased four-fold. But the weight of the ball required to fill the bore will be increased eight-fold; and, as a heavy shot cannot be forced out with the same velocity as a light one, while the combustion of the powder and the generation of the gas continue nearly uniform, it follows that in guns loaded with a large weight of shot per square inch of area, a larger internal pressure will be generated than in guns not so heavily loaded. In a 32-pounder it is reckoned that the shot is moved with sufficient facility to prevent the internal pressure from exceeding 5 tons per square inch; and the gunpowder, it is considered, has all been burned by the time the shot has passed through 24 inches of the bore—this proportion of the stroke, to use the language of Engineers, being performed with full pressure, while the residue is performed expansively. It is estimated that the strain produced in guns by different weights of ball varies as the cube roots of the respective weights; but if hollow shot be used, the bore may be enlarged without any increase of the internal pressure. Hollow shot are objectionable, however, from the large front they present to the atmosphere; and piston shot would be preferable, in which a long bolt would be projected by a piston, or wad, pressed on by the gunpowder gas, but which, from the large frontage it presents to the atmosphere, would fall down while the bolt continued its flight; and such bolt could be supported in the centre of the gun by three or more spiral feathers, like the spiral feather on an arrow, to rifle it in the air. The penetrating power of such a projectile would be increased if its flight were aided by the reaction of a stream of rocket gas issuing system the only material innovation on the model of the old men-of-war lies in the application of iron armour to the sides, whereby shot is prevented from penetrating; and in some cases the armour is not carried to the very bow and the very stern, but only the central part of the sides and a belt at the water-line are protected by armour, and armour bulkheads are carried across the ship, before and behind the protected portions of the sides, so as to form the central part of the vessel into a rectangular fort. This is the principle on which the 'Bellerophon' and other recent vessels have been built, and its advantage is that it enables thicker armour to be applied. The Monitor, or turret system, is the invention of Captain John Ericsson, of New York, an Engineer, as is well known to many members of this Institution, of eminent talents and attainments. In the Monitor system, the sides of the vessel are made very low; and the guns, which are of great calibre, are carried in one or two cylindrical towers of iron of great thickness. About sixty vessels on this system have been built in America; and latterly it has been adopted by Russia and some other Continental powers. The Monitor system is a system of concentration, in which the area of the sides to be protected is reduced to the smallest possible amount, and this small area of armour may therefore be made of enormous thickness without overloading the ship. Such vessels are consequently impenetrable by the heaviest existing gun. But the weight of the broadside is

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behind it; and, although ordinary rockets are most wasteful projectiles, from the large amount of slip they permit, especially in the early part of their flight, yet in the case of a rocket fired from a gun, the velocity at starting will be so great that the slip and waste will be much reduced, while a material increase will be given to the force of the projectile. Nearly all large guns are strained too much; and, therefore, like beams strained beyond their elastic strength, they nearly all break after a certain period of service. But this is merely the effect of faults which are perfectly remediable; and, although the expedients indicated are not propounded as matured or faultless, they nevertheless afford sufficient warrant for rejecting so unsound and dangerous a doctrine as that the maximum power of projectiles has been reached, or, indeed, that there is any visible limit to their power. It would be idle, therefore, to pretend that an iron-clad vessel can be constructed which would remain impenetrable through all future time. But those which are at present constructed should not only be impenetrable now, but ought to possess a margin of superfluous strength. It is easy to change the guns, but it is a far more serious matter to change the ships; and a new ship may be antiquated even before she is launched, unless her designer looks beyond the present balance between guns and armour, and provides a structure likely to remain impenetrable for some considerable time, notwithstanding the increase in the power of guns that is steadily going on. It is not by many light guns that iron-clads are to be pierced, but by few heavy ones. The effect of firing salvos converging to a point has been experimentally tested at Portsmouth; and their destructive power has been found to be much greater than that of the same number of shot discharged successively. But the destructive power of such salvos has also been found to be much less than that of a single shot containing the same weight of metal; and, consequently, few and heavy guns must be accepted, as the only effective measure of offence against armour.

also concentrated into two enormous shot, which have momentum enough to go through the armour of any ordinary broadside vessels. The turret vessels of Captain Coles are an imitation of those of Ericsson, but with certain points of dissimilarity, of which the most material is that the sides of the vessel are not nearly so low as in the Monitors, and the armour of the sides and turrets cannot consequently, with any given displacement, be made so thick. Nor would it be possible with safety to make the sides of Captain Coles' vessels lower than at present, owing to the turrets being carried on rollers on the lower deck, and passing through openings in the upper deck, which it is difficult to make tight without jamming the turrets. The openings to the engine room also consist merely of gratings, or other unprotected orifices, such as are usual and admissible in vessels with a considerable height of side, but which would allow too much water to enter the hold in the case of low vessels. In the Monitors, on the contrary, the turrets revolve water-tight upon a metal ring on the upper deck, and do not pass through that deck at all; and all the usual openings to the interior of the vessel, whether for the admission of air or otherwise, are made either through the top of the turret or through shot-proof trunks or pipes, to the end that even if the deck be washed by the waves, water cannot enter the vessel so long as the deck is water-tight. Captain Coles' vessels have been little tested in actual war; and it is objected that, as it has been found that turrets exposed to blows from heavy shot do not remain round, his turrets, if subjected to this test, would jam in the holes through which they pass in the deck, and the gearing for turning the turrets, and which is attached to the turret walls, would at the same time be thrown 'out of truth,' and would refuse to work. The Monitors in the United States navy were in action, on the average, twenty-five times each during the war, and were several thousands of times struck by shot; and it was found in these vessels that the turret, being driven from a central spindle, continued to work, even if it ceased to be round, while it was never jammed by any of the splinters caused by the breaking up of balls and shells on the face of the armour, and which, it is alleged, would in Captain Coles' vessels be projected into the annular space between the deck and each turret, and prevent the turrets from revolving. The Monitors have been found, during a war of unprecedented magnitude and energy, to be both shot-worthy and sea-worthy. They are confessedly unequalled in their power of penetrating other vessels and of resisting penetration themselves; and there can be no doubt that in any contest between the best broadside vessel in the French or English navy and a Monitor of equal size, the broadside vessel would be inevitably destroyed. The Author does not maintain that broadside vessels may not be useful and even necessary. For attacks upon towns and forts, and for many purposes where a great many small guns may be more serviceable than a few large ones, it can easily be imagined that vessels on the broadside principle may be almost indispensable. But it is maintained that no broadside fleet is safe, unless accompanied by an auxiliary flotilla of Monitors, to protect it from the assaults of other Monitors, which would certainly be sent against it by an enterprising enemy in the first naval war; and further that unless such an auxiliary arm is introduced into the Royal Navy, the broadside fleet, however many guns and ships it may number, will inevitably be destroyed or captured in the first action with Monitor vessels in which it may be engaged. This is not a question of seamanship, or gallantry, or military tactics, but simply of the preponderance of forces; and there cannot be a doubt, that in a hand-to-hand encounter, which must finally determine the issue of any maritime war, the strongest armour and the heaviest guns must necessarily prevail.

As an illustration of the main features of the structure of Monitor vessels, Plate 8, Fig. 1, represents a transverse section of the American war-steamer 'Dictator,' built under contract by Ericsson, the inventor of the system. The extreme length of the 'Dictator' is 314 feet; her beam is 50 feet; and she draws 20 feet of water, with 800 tons of coals in her bunkers, and equipped ready for sea. The 'Puritan,' a sister ship, also designed and built by Ericsson, is 345 feet long on deck, and carries 1,000 tons of coal in her bunkers. The 'Dictator' is fitted with a single turret, carrying two of Rodman's guns, of 15 inches bore. The 'Puritan,' in opposition to Ericsson's advice, was originally fitted with two turrets; but the after turret has since been dispensed with, and the remaining turret is fitted with two of Rodman's 20-inch guns, the service charge of which is from 100 lbs. to 120 lbs. of cannon powder. Ericsson maintains that one turret is better than several, simply because it better promotes the main end of the Monitor system—concentration, whereby heavier guns and thicker armour may be employed than if the same weight of iron were distributed among many turrets and guns. The 'Dictator' is propelled by a pair of engines with cylinders 100 inches in diameter, and 4-feet stroke. The diameter of the screw is 21 feet 8 inches, with four blades, and 34-feet pitch. Steam is supplied to the engines by six boilers, with a double tier of furnaces, numbering fifty-six in all. The heating surface of the boilers is 34,000 square feet, and the grate surface 1,120 square feet. The boilers contain 10,640 tubes, the united length of which is $7\frac{1}{3}$ miles. The chimney is 10 feet in diameter, and 8 inches thick at the base, and is provided with a shell-proof grating, placed about 6 feet above the level of the deck. The engine-room is ventilated by means of a copper fan, of large diameter, suspended horizontally under the deck, and driven by a small donkey-engine, bolted to the deck beams. The fan, which is not enclosed in a casing, draws the air, which it sends into the engine-room, through a pipe or cylindrical trunk, 4 feet in diameter and 8 inches thick, carried high above the deck, and provided with a suitable cap, to prevent water from entering while admitting air. The air thus forced into the engine-room passes into the boiler-room, to maintain the combustion in the furnaces, which is also aided by two Dimpfel blowers, each 78 inches in diameter, applied under the turret, through the top of which the air is drawn. The side of the ship rises only 16 inches above the water, and is defended by armour 6 feet deep and 4 feet thick, 10½ inches of which thickness is iron, and the rest oak. The turret is of iron, 24 feet inside diameter, 9 feet 6 inches high, and 15 inches thick. The vessel tapers to a point at each end, the side armour being continued sufficiently to form a ram in both cases; and by this projection at the stern both the screw and the rudder are effectually protected from injury. The top of the turret is prolonged upwards by a bell-mouthed wall of plate iron, ½ inch thick, so as the better to throw off the water which may dash against the turret; and round the edge of this bell-mouth there is a promenade, fitted with hand-rails and with awning stanchions, to enable an awning to be spread over the top of the turret in hot weather. Behind the turret a narrow hurricane deck, supported by stanchions, extends for some distance abaft the chimney; and from the bottom of this deck the boats are suspended. The ash trunk, through which the ashes are hoisted up when the vessel is at sea, is situated within the chimney; and a door opens from the chimney on to the hurricane deck, whereby the ashes may at all times be discharged overboard from the hurricane deck, even though the main deck should be washed by the waves. The vessel is not fitted with masts; but it would be easy to apply telescopic masts, to enable her to proceed on any distant voyage under sail, so that she might husband her coal until called upon to put forth her full powers. The weight of the shot discharged by the 15-inch gun is 425 lbs.; and by the 20-inch gun, 1,000 lbs. But the main thing which determines the force of the shot is the quantity of powder burnt, which is 60 lbs. in the 15-inch bore, and about twice that quantity in the 20-inch bore.

Referring to Plate 8, Fig. 1, A is the pilot-house, 8 feet inside diameter, 7 feet high, and 12 inches thick, placed on the top of the turret T T, and furnished with elongated sight-holes. The floor of the pilot-house is formed of wooden gratings, through hinged hatches in which the captain and the steersman gain admission from the turret. In the first Monitor the pilot-house was situated near the bow of the vessel, and speaking-tubes communicated between it and the turret; but in action the inconvenience of that arrangement was found to be so great, that Ericsson substituted the arrangement here shown, and which is found to fulfil every required condition—the captain having both the steersman and the gunner under his immediate eye. B B is the bell-mouth of plate iron, for throwing off the water, already referred to; and C C is a strong beam, which rests on a thick collar formed on the stationary pillar D, round which the turret revolves. E E are diagonal braces, by tightening which any desired portion of the weight of the turret may be carried on the central pillar. F is a radial bar on which a small wheel runs, carrying a block and tackle, by means of which a shot may be lifted out of the locker beneath and applied to the muzzle of the gun. G is a toothed wheel, fixed by lugs to the gun-slides, which are in turn fixed to the turret; so that by rotating this wheel, which is done by appropriate gear worked by a donkey-engine—omitted in the diagram for the sake of simplification—the turret is turned in any required direction. P is the port through which the gun is fired; and S is the port stopper, which consists of a strong piece of cranked iron, supported at the ends by short, thick gudgeons; and by turning this crank in one direction the port is shut, while by turning it in the other direction the gun may be protruded past the cranked part. W is the steering wheel, and R a rack formed of steel, by which the motion of the wheel is transmitted to the steering barrel. This rack slides in a groove formed in the pillar D. The beam C C carries rafters supporting iron bars 4 inches deep, 3 inches thick, and placed 2½ inches apart, on the top of which are laid perforated plates of iron, 1 inch thick, covering the whole top of the turret. The turret revolves on a flat wrought-iron ring let into the deck; and to the bottom edge of the turret is bolted a similar ring of gun-metal, which makes, with the iron ring below, a water-tight joint. Oil holes are provided to facilitate lubrication. A small trough outside the base of the turret is filled with oakum, and a similar small trough within catches any leakage which comes through the joint, and conducts it through scupper-pipes to the bilge.

Such, then, is the ‘Dictator;’ and if the destructive and resisting powers of a vessel of this kind be compared with those of the best broadside iron-clads, like the ‘Bellerophon,’ it is not difficult to see which must necessarily prevail in action. The ‘Bellerophon’ carries on each broadside, five guns of 10½ inches bore, besides two guns at the bow and three at the stern, of 7 inches bore. None of these guns could pierce an iron turret 15 inches thick, or low sides, 10½ inches thick, of iron, backed by upwards of 3 feet of solid oak, supported by a thickness of 50 feet of armour-plated deck and beams. The deck of the ‘Dictator’ is composed of oak planks, 9 inches thick, placed on oak beams, 15 inches deep, pitched very close together, and the top of the deck is covered with 2 inches of iron. Plate 8, Fig. 5, showing the maximum elevation and depression of the heaviest guns of the 'Bellerophon,' indicates that the greatest possible angle with the horizon, permitted by the ports and beams, at which the 'Bellerophon' could fire down upon the deck of the 'Dictator,' to be 10 degrees; though how the recoil is to be taken with so much depression is not very clear. Now, experiments have been made in America to ascertain the effect of shot fired from an 11-inch gun, at an angle of 15 degrees, against a weaker target than the deck of the 'Dictator,' and in no single instance did the shot penetrate, although it very much damaged the target. It is clear there is some law, according to which the thickness of armour may be reduced, as the angle of incidence is diminished; and, so far as the best existing knowledge will permit, this law has been applied in the case of the 'Dictator,' and the result is that all parts of the vessel are equally strong to resist the forces which may be brought to bear against them; and none of the Monitors which have been engaged in America against forts and ships have, so far as can be ascertained, been seriously damaged in the deck.

It may be supposed that the 'Dictator,' being a very low vessel, would be easily run down by a vessel rising higher out of the water; but when the ram bow of the 'Bellerophon' is applied to the side of the 'Dictator' (Plate 8, Fig. 3), even if the 'Dictator' be supposed to remain quiescent on the water, and to allow the 'Bellerophon' to run at her broadside, it will be evident that the effect of the ram bow of the 'Bellerophon,' acting on the corner of the armour shell of the 'Dictator,' which would be the point encountered, would be rather to raise the 'Dictator' in the water than to sink her, while the point of the ram would not touch the side of the 'Dictator' at all, and the corner of the armour shelf, 4 feet thick, would inflict more injury than it suffered. Indeed, it has been found in practice that the 'Merrimac,' when she encountered the first Monitor, and tried to run over her, suffered far more damage from the attempt than she inflicted upon her opponent; and, in any repetition of the attempt in a contest between such vessels as the 'Bellerophon' and the 'Dictator,' the same consequences would probably follow. It is unwise, moreover, to trust to the expectation that, in any naval conflict, a vessel will lie still, to let her opponent run into her in the most convenient manner. The 'Dictator,' it must be remembered, is a ram vessel too, with a ram at each end, formed of a prolongation of the armour belt, 4 feet thick, which goes right round the ship; and if the contest is to be determined by efficacy of ram power, she certainly would not be found to be, in that respect, inferior to her opponent, since she could run into the hull of her opponent, whereas her own hull is fendered off from retaliatory attack by the armour shelf extending round the vessel. It is, however, by the power of the guns and the thickness of the armour that the issue of the contest would be mainly determined; and while the guns of the 'Bellerophon' would be powerless against the armour of the 'Dictator,' even if fired in converging salvos, the 'Dictator's' guns would easily pierce the armour of her opponent. But the 'Dictator' is the smaller vessel; and if her displacement were to be made equal to that of the 'Bellerophon,' the turret might be made 24 inches thick, and the thickness of the other armour and the power of the guns might also be proportionately increased. To compare two systems together, vessels of equal size must be taken; and it is easy to see that such a Monitor would work an amount of destruction which would speedily sink or disable the broadside vessel. In Admiral Porter's account of the attack upon Fort Fisher, it is stated that the fire of the Monitors, though irresistible, was slow. No doubt the fire from heavy guns will always be slower than the fire from light ones. But the main cause of the slowness of the Monitors was due to the necessity of releasing, at every round, the screws which tied together the slides of the gun-carriage, which, by their convergence towards the breech, gradually take up the recoil by friction; and in large guns these screws are necessarily heavy and difficult to handle and undo. This fault, however, has since been corrected by Ericsson, by the introduction of what is called a revolving compressor. The gun, when loaded, is brought forward to be fired by a winch handle giving motion to wheelwork gearing into racks formed on the gun-slides. When the gun is brought forward, the winch handle is locked by a suitable detent; but a friction coupling is interposed between the handle and the pinions which gear with the rack, the friction of which coupling is so regulated by screws as to be sufficient to bring forward the gun without slipping; but when the gun is fired off while the handle is locked, the force of the recoil expends itself in overcoming the friction of the coupling, since the gun runs back with such a resistance in overcoming the friction of the coupling, as exactly to use up the force of the recoil. The amount of friction in the coupling, and, as a consequence, the amount of recoil in the gun, may be regulated to the exact point required, by tightening or loosening certain screws affixed to the coupling for the purpose of making the friction greater or less. By this arrangement, the time heretofore lost in undoing heavy screws is saved, and the fire of the gun is made more rapid than before. The 'Dictator' is fitted with this improved gun-carriage, as also are many of the recent American broadside rovers, such as the 'Madawasca'—vessels of a high speed and with a heavy armament, but without armour.

The main point connected with the structure of the Monitors, which has provoked controversy among naval men, is whether it is possible to make heavy vessels, so low in the water as the Monitors are, safe at sea. Even if this should be doubted, the necessity of the employment of Monitors for the protection of ports, harbours, and estuaries, is not the less exigent. But although, in the nautical mind, the ideas of seaworthiness and height of side are indissolubly associated, it will not be difficult to show that there is no necessary connection between these conditions. No doubt ordinary steamers, with open crank hatches, and with various other unprotected orifices communicating between the deck and the interior, will be quite unsafe if sunk very low in the water; for the waves will, in such vessels, dash over the deck, and the water which runs into the hold through the open hatches may quench the fires, and finally sink the ship. But a totally different state of things will supervene if the deck be made as tight as the bottom, and if the only openings leading to the interior are made through high towers which the waves cannot enter. Now, the Monitors are constructed on this principle. The deck is made perfectly water-tight in every part, and the air required for ventilation, and for maintaining the combustion in the furnaces, is drawn partly through the top of the turret, and partly through high shot-proof trunks provided for that purpose. In point of fact, the Monitors do rise to the sea; and the 16 inches of side given in the 'Dictator' represents an ascentional force of about 500 tons. But even if the Monitors did not rise to the sea at all, but were as immovable as a half-tide rock, they would, nevertheless, not be unsafe, if their breathing-towers were carried to the height proper for that condition. The maximum height of waves does not exceed about 30 feet; and a tower which rises to a height greater than the waves, though it may be swept over by the spray, will not be submerged. It is found in practice that towers of the height of those of the 'Dictator' are quite adequate to enable the vessel to encounter with safety the heaviest seas to which any vessel can be subjected. Commodore Rodgers, of the United States Navy, reports on the performance of the Monitor 'Weehawken' in a severe gale, with the waves 30 feet high, as follows:—"During the heaviest of the gale I stood upon the turret, and admired the behaviour of the vessel. She rose and fell to the waves, and I concluded that the Monitor form had great sea-going qualities. If leaks were prevented, no hurricane could injure her. I presume in two days we shall be ready for any service, as we need no repairs, and only some little fittings."[1] During two years the Monitors were exposed, on a stormy coast, to all kinds of weather, and proved themselves to be both shot-worthy and sea-worthy, and the healthiest vessels in the American fleet. The voyages, however, of the 'Monadnock' round Cape Horn, and of the 'Miantonomah' across the Atlantic, have afforded greater evidence of seaworthiness than any engineering considerations, and thus the most plausible of the objections to the Monitor system has now been abandoned.

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[1] *Vide* "Report of the Secretary of the Navy in relation to Armored Vessels," 8vo.; Washington, 1864, p. 46.

Of the sixty Monitors in the American navy, a few have been lost by coming upon torpedoes or otherwise. The 'Weehawken,' after having successfully encountered the roughest seas, sank in Charleston Harbour, in consequence of a large hatch in the deck having been by some oversight left open, when a heavy gale came on, and the water which swept over the deck entered the opening and filled the ship. The first Monitor—a vessel hastily constructed—sank at sea, owing to a leakage round the base of the turret; while the centrifugal pump, which had been chiefly relied upon for discharging overboard any water that leaked in, owing to some imperfection, would not work. After the vessel had been in use for some time in the Southern States, she was found to require repairs; and among other defects, the turret joint being leaky, the captain, with the view of staunching it for the voyage to New York, lifted the turret and spread oakum beneath. The oakum, however, was lumpy; the turret rested only on the lumps; and when the vessel went to sea, the loose oakum was washed out of the joint, and the leakage was thus greatly increased, and overpowered the pumps. The same thing, however, would have occurred in any vessel whatever, if, from any cause, more water leaked in, through some accidental fissure, than the pumps could discharge. This accident has been put forth as a demonstration of the necessarily unseaworthy character of vessels on the Monitor construction; whereas if, instead of introducing oakum beneath the turret, the joint had been made tight by painting it over with a mixture of white lead and tallow, the accident would never have occurred. If Monitor vessels are made tight, they may be navigated in the most boisterous seas with safety; but no vessels, whether of the Monitor or any other construction, if the workmanship is bad, the joints leaky, or the details of equipment neglected, can be sent to encounter the perils of the deep without a corresponding amount of danger. The want of liveliness imputed to the Monitors, instead of being an objection, is a material advantage in any vessel requiring to take an aim with heavy guns, since it makes the aim more sure. It is related by one of the Monitor captains, that, when sent to sea in company with another vessel of the ordinary kind, and which was accounted an excellent model, the men on the deck of the common vessel were hardly able to keep their feet, even with the assistance of life-lines, the sea was so rough. The Monitor vessel, however, though exposed to the same rough sea, neither rolled nor pitched to any material extent. Of course, the larger the

Monitors are, the more distinctly will this property of superior steadiness be manifested; but even in small Monitors it is exhibited in a remarkable degree. It is plain that, whereas ordinary vessels, in rolling heavily, may expose the portions of the bottom uncovered with armour to the enemy's fire, there is no such risk in the case of the Monitors, seeing that the amount of their rolling is so small.

It has been objected to the Monitors that, being almost submerged vessels, they cannot have any light and cheerful cabins in which the officers could live. With such sentimental grievances the Author does not feel called upon to deal. In the 'Miantonomah' the officers' quarters appeared to be comfortable enough. But it is sufficient to know that the quarters provided for the crew are airy, wholesome, and adequately light, and that the Monitor vessels are popular both with officers and men. The Secretary of the United States Navy, in his Report to Congress in 1865, on the health of the fleet, says, "There was less sickness on board the Monitor vessels than in the same number of wooden ships with an equal number of men, and in similarly exposed positions." And, after recapitulating some of the particulars, he adds, "No wooden vessels in any squadron throughout the world can show an equal immunity from disease."[1] There would be no difficulty, in such a vessel as the 'Dictator,' in putting glass windows all round the top of the turret, so as to form it into a circular cabin, like the lantern of a lighthouse—the windows being made in removable panels, which might be taken away before going into action. On points of this kind every captain might be left to please himself. But what it concerns the government and the country to know is, that in the Royal Navy of Great Britain there are vessels of at least equal powers in guns and armour to those possessed by any other nation; and that, in the event of a naval war, the broadside fleet will not be disabled or captured from the want of a flotilla of protecting Monitors, whose function it would be to encounter the Monitors which the enemy would assuredly employ.

The Paper is illustrated by a series of diagrams, from which Plate 8 has been compiled; the Institution of Naval Architects kindly allowing the use of their copperplate engraving of the section of the 'Dictator,' Fig. 1.

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[1] *Vide* "Report of the Secretary of the Navy, &c.," 8vo.; Washington, 1865, pp. xix. and xx.

Mr. J. Bourne said, since he had given attention to this subject, public opinion had overtaken the doctrines expressed in the Paper; and he was pleased to see, by a report of a lecture recently delivered at Plymouth, that Mr. Reed had arrived at somewhat similar conclusions. He thought, whatever opinions might be formed on these doctrines, there was one conclusion in which all would agree,—that at this Institution such a subject as the proper construction of Ships of War could be better considered and better dealt with than anywhere else, it being, in fact, the most authoritative tribunal before which any such question could be brought.

Mr. E. J. Reed (Chief Constructor of the Navy), in reply to the Chairman, said his object in attending was to listen rather than to speak, and he hoped he should be excused from entering into the discussion, at any rate at its present stage. He must say, in reference to a remark from Mr. Bourne, that with many of the principles enunciated in the Paper he fully concurred: at the same time, there were some statements to which he begged leave to take exception. But on this point he agreed with him,—that it would be just as unwise to neglect the meritorious points of the Monitor system as to rest satisfied with raising more or less imaginary objections to it. He also thought, in proportion as the necessity for increased armour was occasioned by the progress of artillery, so the necessity for cutting down the superficial extent of that armour, and approaching the Monitor system, would be rendered obligatory, if reasonable limits, as regarded dimensions, were adhered to. At the same time, he had much hesitation in assenting to the doctrine that it was justifiable to go to the lengths indicated in the Paper upon the 'Dictator.' He was not prepared to agree to a reduction of the sides of a vessel to a margin of 16 inches above the surface in so large a ship. There were other points to which he might allude later in the discussion: he should never attempt to go over the entire Paper, because to do that it would be necessary to frame counter-statements from official American reports, which were not altogether on the side on which they had been quoted.

Mr. J. Scott Russell, V.P., agreed with Mr. Bourne in his high appreciation of the American Monitors, and eulogised the talent and judgment of Ericsson, who had created these Monitors. Ericsson was an early competitor in the race of mechanical and civil engineering in this country; and he had found in America the reward due to his energy, his very high talents having been recognised by the late President, Mr. Lincoln.

The extreme difficulty of the problem of an armour-plated ship was this: The armour must be thick enough to resist the guns of the enemy, however powerful; and the ship must be big enough to carry all the armour, in addition to everything else that a

Fig. 1

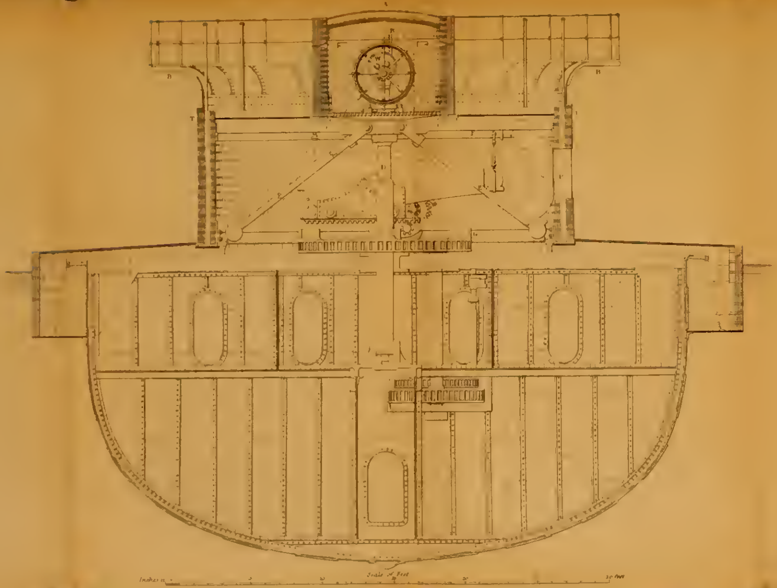


Fig. 2

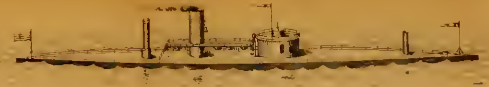
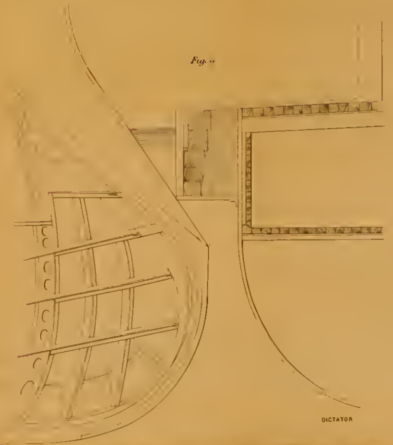


Fig. 6



BELLEROPHON

Fig. 1.

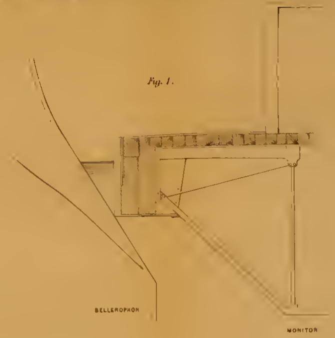
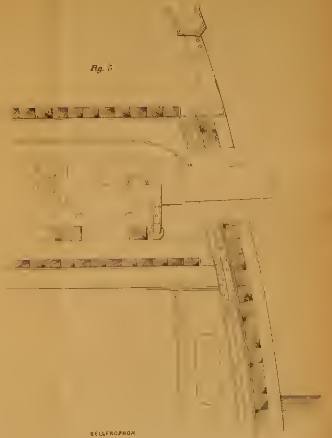


Fig. 5



BELLEROPHON

SHEWING MAXIMUM ELEVATION AND DEPRESSION OF GUNS

ship of war required. It was therefore evident that, if the thickness of the armour of a war ship, and the quantity of surface above the water which was to be made shot-proof, were given, the size of the ship was also given, and nothing could diminish it. Hence it was, that, at starting, very big ships were built, because it was decided beforehand how many guns they should carry, how high those guns should be out of the water, and how thick the armour should be. If it were now said an armour-plated ship must be built to carry the thickness of armour-plating and of backing recommended by the Author, as well as the gun-battery the same height out of the water, exactly of the same shape, with the same number of guns, he could not say how immensely bigger than the 'Warrior' that ship would have to be. There were, however, other classes of ships—but he believed the 'Warrior' type and the 'Bellerophon,' as Mr. Bourne said, might be taken to represent those which were likely to be used in the Royal Navy. Speaking of the former, he must be allowed to say the 'Warrior' was the first of a series of trials, and that the improved 'Warrior' was the 'Achilles;' therefore, when he spoke of the 'Warrior' he meant the improved 'Warrior' of the 'Achilles' kind, because it had been originally said that the 'Warrior' could not carry the quantity of armour without being unstable at sea. That being an established fallacy, very properly the 'Achilles' was built with armour from end to end, while the principle of limiting the battery was retained. Vessels of the 'Bellerophon' class obtained flotation within moderate size, by shortening that part of the vessel which constituted the shot-proof battery, and that was one mode in which any thickness of armour could be carried on a moderate-sized ship. That he would call the method of limited battery, because it might be contrasted with the Monitors. The Monitor class enabled a short ship to be made with much thicker armour than a ship of what he called the English class. The reason was obvious. The height of the vessel out of the water was reduced, for the express purpose of diminishing the quantity of the armour, of which there had been so enormous an increase in the thickness. That was substantially the essence of the Monitor ships as distinguished from others: they carried the least height out of the water, and had the indispensable thickness of armour. Now, to convert a Monitor into one of the English class of ships, it would be necessary to raise the sides from about 30 inches to something like 6 or 7 feet out of the water before reaching the gun deck; therefore, instead of having the 6-inch stripe, there would be that and another 6 or 7 feet above all the way, which would give greater top weight, and the quality of the vessel would be altered. Above the gun deck there would have to be put the battery, consisting either of a turret, or what, in the English class of vessels was called the limited battery. That could not be done without immensely enlarging the ship; but with an enlarged ship a much greater number of guns might be put within the shot-proof battery. There was another feature not much alluded to in the Paper, but which he had the pleasure of admiring in the 'Miantonomah,' which was an admirable specimen of the class. It had two turrets, and the high hurricane deck over the centre of the ship; and this high deck in the 'Miantonomah' was a very convenient deck. It had davits underneath for the boats, and all the conveniences and comforts of an upper deck. It was in this way provision was made for the conveniences, though some of the comforts of a dry deck were resigned. He did not know how many people would like to fight that ship with the sea running 6 feet over the deck, but there were plenty of sailors who would fight the 'Bellerophon' or the 'Warrior' in a sea which would run 6 feet over the deck, and sweeping it from end to end. If that were so, then he begged Mr. Bourne to draw a distinction between a sea-going ship that could cross a sea and fight at the end, and a sea-going ship that was able to fight at any point of her voyage where she might be required to do so. He did not think one of these ships could be sent out alone from Portsmouth to the Cape of Good Hope, and the crew asked to be ready, in any weather, to meet one of those big sea-going vessels which would be sent against them. Nevertheless, he thought them excellent vessels for inland seas, and long rivers; where there was smooth deep water, and plenty of it; and neighbouring ports where coals could always be had. These Monitors had been well called floating gun-carriages. It was the idea of the inventor, that all that was asked of him was to make floating gun-carriages that would be capable of moving themselves from place to place, and then, in smooth water, of doing certain work. That was what the Monitors had done perfectly; the idea of making them fit to go to sea was an admirable idea, and had been admirably accomplished. And this floating gun-carriage, even in a certain size of waves, had the quality of steadiness which no ordinary ship could possess, for the reason that the sea lifted the one ship very much, while it lifted the other ship very little. Therefore, the Monitor form was the best possible for the defence of harbours, such as Spithead, where there was little sea running; and yet such a floating gun-carriage could be moved by her own steam, and placed where she was wanted, and at the same time be got out of the way of a discourteous enemy. He did not think he was betraying official confidence in saying, that when a certain commission was appointed by the Government, to study what sort of vessels would best defend inland waters, they did design for that purpose vessels as low in the water as that, over which the water was permitted to flow freely, and on which there were only limited elevated batteries. He thought that for batteries in smooth water with impenetrable armour, that principle of a low deck over which the waves might pass freely, without the raised side, which would lift them out of the water, was a very good principle.

With regard to the relative merits of the Monitor form of turret and that of Captain Cowper Coles, the former being entirely above the deck, the strength of the deck remained uninjured, and the whole space below the deck was left clear. That was a great advantage. There was another advantage which Mr. Bourne had touched upon, that the turret sealing the deck, left it water-tight. The friction on the deck plates was diminished by the screwing up of the sustaining rods, so as to balance the weight of the turret upon the lower pivot, as in an ordinary well-constructed railway turntable pivoting upon the centre below. He thought it must be admitted that the whole mechanism of the turret was exceedingly good. On the other hand, the turret of Captain Cowper Coles had the advantage of not being so high above the deck of the vessel. But if the question were that of reducing and lowering the decks, similar to what had been done in these vessels, then he agreed with Mr. Bourne in preferring to have the turret completely above the deck, leaving the deck perfectly entire. He thought that a combination of the turret with the broadside system was better for an English sea-going ship than any plan that had yet been executed. He knew vessels of this kind had been contemplated, though he did not know it had ever been resolved to execute them; but if a vessel were built so that it could carry two turrets at a distance of 120 feet from each other, there were the means, by merely connecting those turrets by two longitudinal iron battery faces, of adding, on both sides, a large number of broadside guns of as heavy a calibre as could possibly be managed, whilst extremely little would be added to the bulk and weight of the ship. Such a vessel would be very powerful and large—possibly larger than the 'Bellerophon,'—and his own belief was that that kind of ship would be adopted—viz., a long central battery, terminated by two of those turrets. One reason for that was,—that when there were two turrets, one of them was so much in the way of the other, that they could never both fire parallel to the keel in the same direction. Their fire being thus limited, the distance between them could be used for a broadside battery. Mr. Bourne had said that it was the fault of the Monitors that they fired so slowly; this was at once remedied if guns were interpolated between the two turrets; and it was quite plain broadside guns could be used for a multitude of purposes with advantage. It seemed to him, therefore, that if this Paper did nothing more than compel due attention to be given to the value of turrets, it would have done much good.

Captain J. Selwyn, R.N., rose with some diffidence to bear testimony to the extreme accuracy and justness of the views which Mr. Scott Russell had advanced. They were, with few exceptions, those which any intelligent seaman would have brought forward; but they, perhaps, did not touch upon one or two points to which he would advert. One of the most important things asserted with regard to the turret and broadside systems and the comparison between them was, that the turret was capable, by means of its power of carrying heavier guns, of firing an equal weight of metal with that which could be fired from a broadside ship with ordinary broadside guns. But an important element was omitted in this calculation, which was this: that all naval wars had been settled by close action; that that close action had usually been accompanied by the necessity of firing both broadsides; and if both broadsides were coupled together, the turret very materially failed in the comparison. There was no reason to believe that the conditions of naval warfare in regard to large fleets were materially changed, except that motive power was employed to enable ships to manœuvre as an army manœuvred on land, instead of doing so only as the wind permitted. British seamen would always seek close attack, and would refuse to waste shot and time in fighting at a distance on the water. No great naval action had ever been settled at long range; and he had the authority of the oldest and most distinguished members of the profession for stating, that they did not see their way to the settling of a naval action by long shots, whatever steadiness of platform might be attained. There was another point to be considered with reference to the ram bow of the 'Bellerophon' and of all the broadside ships when impelled against a vessel like the 'Dictator.' If she were supposed to float in still water, that would give a different result from that obtained when the same vessel was following, as all Monitors would do, the inclination of the sea-wave; the distinction between a vessel of ordinary construction and a Monitor, which was merely a floating raft, being, that the one pitched and rolled whenever the sea was rough enough, while the other took the inclination of the sea-wave. There was thus steadiness in the gun platform in a broadside ship under sail; while in the other case—the Monitor—there was great unsteadiness in fighting in a breeze in heavy weather—what was called 'seaman's weather,' when he delighted to see his ship doing her best under circumstances in which many of the Monitors had thought it necessary to put before the wind and run away. There was also the fact to which Mr. Russell had adverted, of the upper deck platform being abandoned to the waves. It was a great mistake to suppose the British navy could afford to be composed entirely of floating gun-carriages. Amongst the many objects for which that navy was sent to sea, the one most infrequently fulfilled was that of fighting an action. Happily, many years of peace were succeeded by a few years only of warfare, and the proportion of peace to war had, still more happily, of late years, been constantly on the increase. The carrying of troops in a Monitor ship, which was a duty many vessels had to perform, was utterly impossible when there was a platform which was swept by the waves; and what might be good ventilation for a limited crew, would be very bad for a ship crowded with troops. Again, in the matter of blockading the ports of an enemy, he did not think the 'Miantonomah' was the sort of vessel in which a long blockading service could be well performed. With regard to what had been termed the sentimental grievances of sailors, he would say, those who had lived a long time together on board ship recognized the fact that there was such a thing as having a comfortable house; and he was sure his brethren on the land would not wish to curtail the comforts, which were usually sufficiently small, of those who went to sea.

Mr. Charles F. Henwood said, in the absence of Captain Cowper Coles, he was sorry Mr. Bourne had brought forward the question of the priority of the invention of the turret system; but as that was a discussion not suitable to this place, he would not enter upon it. The subject had been treated as if Captain Coles had designed one kind of ship, and Mr. Ericsson another; whereas, in fact, the principle of Captain Coles' invention was the substitution of central batteries for broadside batteries. He had not proposed any particular form of ship as an universal type, but merely a method of arming and fighting the ship. It was not a question of mechanical construction of the turret, but simply the principle of armament. There were many points on which he agreed with the Author, and others to which he took exception. With regard to the power of the 15-inch guns, Mr. Bourne stated that the ordinary charge of powder was 60 lbs.; whereas 30 lbs. was the ordinary service charge, and no gun was allowed to fire more than 60 lbs., and that only for a limited number of rounds. He thought precautions should be taken to avoid going too far in the thickness of armour necessary to repel the shot.

Mr. N. Barnaby said, through the Secretary, that the main point of the Paper appeared to be the advocacy of very thick armour and backing, 18 inches or 2 feet of iron, and 4 feet of oak. The Author considered, and Mr. Barnaby thought rightly, that no vessels but those built on the Monitor type could carry such armour, and the Author therefore insisted on the necessity for constructing a flotilla of such vessels. Mr. Barnaby begged leave to point out, with regard to these views, first, that vessels of the Monitor type were not sea-going. He did not mean to say that they behaved badly in a seaway, but that they could not maintain an independent existence on the sea. They were not masted, and they could, under no circumstances, be worked as sailing-ships. Any attempt to fit them with masts and sails would show that they required to be transformed to the English type of turret-ship, which could carry no heavier armour than a broadside ship. If it be said that the absence of sail-power did not disqualify a ship for maintaining an independent existence on the open seas, because she might rely on her steam power, the answer was, none of the ships of the Monitor type carried sufficient fuel to do this. The smallest ship referred to by Mr. Bourne would require her engines to develop something like 1,000 indicated H.P. at least, in order to obtain a speed of 5 knots or 6 knots per hour, which would be got from sails. This would cause a weekly consumption of about 200 tons of coal, or for six weeks' independent existence, 1,200 tons. Now the Monitors did not carry one-half as much. If then the Monitor type were adopted, it would be necessary, in order to make it sea-going, to save from the armour what was required to be put into fuel, at least so long as the fuel was limited to coal, or heat-givers of equal weight with coal. If both conditions were to be satisfied; if the vessels were to be sea-going. and also be capable of carrying the armour recommended, then far larger ships would be required than appeared to be contemplated. The dimensions of the 'Bellerophon' had been referred to; but such a ship built as a Monitor could not find stowage room for one half the coals she would need, without saying anything of her want of power to carry the weight of them, with increased armour. It might be said that, to avoid this difficulty, each sea-going Monitor should be accompanied by a steam-collier as a tender. But this course might be very perilous; and if contemplated, it would be necessary to meet the objections which sailors would immediately bring against it. Mr. Barnaby assumed in these remarks that the Author was speaking throughout of sea-going ships; for otherwise the Paper did not touch the real question at issue between broadside and turret principles in England.

All accepted the American Monitor, without dispute, as the best coast-defence ship that could be devised, providing only some modifications were made, to suit local circumstances and national peculiarities. The conclusion, then, at which he arrived was, that in sea-going Monitors, of moderate size, the armour should not exceed one-half the thickness named by the Author, which would be sufficient to keep out shells at short ranges; and while this could be done, and the artillery carried by the sea-going ships of foreign navies could be resisted, he thought that it was comparatively unimportant to be penetrable to the few guns carried by vessels at low speeds, and which vessels could not keep the sea. There was another reason, of the highest importance, as he thought, why fleetness and sea-going capacity should not be wasted on 'piled up' armour and backing on the upper works of a ship. It must be borne in mind that, however thick the armour, the bottom of the ship was left as weak and defenceless as ever. It must not be forgotten that there were such things as submarine guns and torpedoes. Both France and Russia were at this moment considering well-devised plans of working guns below water, which would sink any of the proposed heavily-armoured ships. Such ships, too, would be helpless against torpedoes worked from boats on the open sea. Torpedo boats, made proof against almost any firing which could be directed against them, could be carried in sea-going ships, and doubtless would be so carried and employed. In view of these new and undeveloped, but terrible agencies, it was desirable not to create a large fleet, like that recommended, at the cost of many millions of money, which, although secure from gunpowder above water, might easily be rendered obsolete and useless, because undefended against gunpowder fired from beneath.

Mr. E. J. Reed said, after the wish expressed by the Chairman, he felt it his duty to make one or two remarks, which he would do as briefly as possible. He would say at the outset, it was not consistent with his feelings, nor would he presume, under existing circumstances, to enter upon the general discussion of the question as between broadsides and turret ships, because that was a subject on which he had said all he had to say four years ago, and he had not materially altered his opinions upon it since that time. He proposed to make a few observations on Mr. Bourne's Paper, and first, as a mere incident, to correct one point, with reference to the comparison between the 'Bellerophon' and the 'Warrior,' which vessels were named in connection with each other. As to the weight of armour carried, he would mention that the 'Bellerophon' had a double iron skin $\frac{3}{4}$ of an inch thick, which the other vessel had not, and this was a circumstance that should be remembered in comparing the strengths of their sides.

Upon the general question he agreed on the whole with Mr. Bourne's views, as expressed towards the end of the Paper, believing that it was right, this country, with its resources in coal and iron, should not fall behind other powers either in strength of armour or strength of guns, nor in the speed and coal-carrying power of its ships. He was therefore a little surprised to find that the Author was content with drawing a comparison between the American vessel the 'Dictator' and the existing ships of the Royal Navy. He admitted that heavier guns and armour had been used by foreign powers than had been thought necessary in this country; but he considered the reason for that would be found rather in the fact that in America they had been compelled to use thin iron plates in place of solid armour, and had preferred heavy guns and light charges to lighter guns and heavier charges. There were two or three minor points about the 'Dictator' which he would allude to. He thought it would be difficult to put masts into that ship for two reasons:—first, because the deck being already within 16 inches of the surface of the water, she was not in a position to receive masts and sails, and all those stores which formed the concomitant features of a sailing ship; and next, because if the displacement were equal to that duty, the stability of the vessel would not be so. It was obvious, to those acquainted with the circumstances of a sailing ship, that a primary condition of such a vessel as the Monitor must be the sacrifice of a very considerable amount of statical stability; and that, even under a moderate spread of canvas, she had no means of resisting the inclining force that would thereby be brought upon her. That would apply to all vessels of the Monitor class, but especially to those having a free-board of only 16 inches, and even that reduced by the weight of masts and rigging spoken of.

He was surprised to find it argued that the 'Bellerophon' could not ram the 'Dictator;' because both the diagram and the argument employed showed that she could do so. Supposing the 'Bellerophon' approached the other vessel with any speed, something must yield; and the argument was, that the yielding would consist merely in the raising of the 'Dictator's' side out of the water. He admitted that might be so; but he would ask whether the lifting of the armoured side would not favour the penetration of the hull below? He should be sorry to admit the argument, and act upon the assumption that the bow of the 'Bellerophon' could not ram that ship: his own opinion was it could,—at all events under favourable circumstances. He, however, did not insist greatly upon that point; but he alluded to it for another reason, viz., that one of the features of the Monitor system, with a very low free-board, consisted in the great danger of foundering, which must result as soon as penetration took place in any part of the vessel. With a free-board of only 16 inches, the 'Dictator' would require to be divided into longitudinal compartments about 20 feet long, to keep her afloat with a perforation in her bottom; and the nature of the vessel was such as to make it impracticable to divide her into water-tight compartments 20 feet long, as the whole of the living accommodation was to be below.

Coming now more nearly to the main question, he repeated he was surprised to hear so much stress laid upon the 'Dictator,' because if the information Mr. Reed had received about that vessel was correct, the 'Dictator' was not a vessel signally in advance of British vessels with regard to resisting-power. There was a quality about the diagram of the 'Dictator' (Plate 8, Fig. 3) which did not exist in the diagram he had received from

America, and it was a quality which he considered gave advantages over those which the 'Dictator' possessed. He had been furnished, by a gentleman who was in a position to get at the truth, with a section of the 'Dictator' (Plate 9, Fig. 1), and he should be happy to learn that it was not a correct section. It would be seen that while the 'Dictator' was said to be plated with $10\frac{1}{2}$-inch armour, there was only a short depth of the overhanging side that was plated to that extent. There was a free-board of about 18 inches; and for 15 or 18 inches above, and 7 or 8 inches below the water, the ship was plated with a series of six plates, each 1 inch thick, behind which were three strakes of iron $4\frac{1}{2}$ inches thick, making the total thickness of iron at that part $10\frac{1}{2}$ inches: but it would be observed that the $4\frac{1}{2}$ inch armour ceased at about 8 inches below the water, where it was only about 6 inches thick; and at 2 feet below the water it was only 3 inches thick.

Mr. BOURNE begged to state that the diagram to which Mr. Reed was referring was not correct. His own diagram (Plate 8, Fig. 3) was obtained from Mr. Ericsson, who might be considered the best authority on the subject; and in so far as Mr. Reed's diagram was inconsistent with that, it was incorrect.

Mr. REED was pleased to receive that assurance, because he should have considered it a lamentable thing if the 'Dictator' had been held up as a model of a war-engine, when the armour was but 3 inches thick 2 feet below the water. He was disposed to believe that the drawing which Mr. Bourne had furnished to the Institution was correct, but he thought it must be of a later date than his own; and he was led to think so because he had been informed that, whereas the 'Dictator' was designed to carry 1,000 tons of coals, and to steam at a speed of nearly 16 knots an hour, as a matter of fact the 'Dictator' was not capable of carrying more than 450 tons of coals, and at a speed so much below 16 knots that he was afraid to mention it. With all these concessions the Paper seemed to corroborate what he had been told,—that was, that the original water-line was to have been 2 feet below the upper deck, while 16 inches was the free-board now put forward, and probably by the six plates each an inch thick it had since been carried down. He did not for a moment suppose that a man of Mr. Ericsson's ability had made a mistake in his original calculations, but Mr. Reed knew enough of the construction of war-vessels, and could imagine enough of the pressure which was put upon designers in time of war, to believe that those modifications of the original designs which he had pointed out might have been made. However, accepting Mr. Bourne's diagram in all its strength, it fell at once out of all comparison with the strength of the English iron-clads. The whole depth of the armour-plated side was 6 feet, and, allowing 16 inches for the free-board, there would only be

4 feet 8 inches of armour-plating below the water. That was a less depth of plating than existed on any iron-clad frigate in the British navy: it was 1 foot 4 inches less than on most of the later frigates; and when from any cause the armour had been reduced to a depth of only 5 feet below the water, he had felt that an immense sacrifice was made, because the water-line, however useful in still water, was a mere fiction and a chimera when a vessel went to sea. If the Americans were content with armour 4 feet 8 inches below the water-line, and with making the greater part of that to consist of six separate plates, each but 1 inch thick, placed one upon another, so far from accepting that as a plan for the Monitors of this country, he regarded it as a degree of retrogression which he would be sorry to have part or lot in.

In order, in searching for increased armour and guns, not to proceed upon false premises, he had prepared a diagram of the side of the 'Hercules' frigate (Plate 9, Fig. 2), from which it would be observed, that the thickness of armour at the water-line was 9 inches, and that that thickness of plate was extended further, or as far below the water as the six separate inch plates in the 'Dictator,' while the lower plating was 6 inches in thickness, the whole of which was backed with teak varying from 9 inches to 12 inches in thickness. Behind that there were two $\frac{3}{4}$ inch iron skins, upon the 10-inch frames of the ship, adding $1\frac{1}{2}$ inch to the armour. Between those iron frames teak was worked in vertically, and within that another pair of layers of teak inside, and behind that there came a $\frac{5}{8}$ths iron skin, which was backed with 7-inch iron frames. The armour extended about 6 feet below the water; and he had no hesitation in saying, in the presence of any advocates of the American Monitors, that there was nothing in the American navy which approached this for power of resistance, whether as to weight of armour or as a combination calculated to resist shot. There could not be a doubt, in the minds of those who had followed the experiments made in this country, that a succession of thin plates could not be compared with armour in the solid, and that the employment of a series of plates, each 1 inch thick, was not a subject of instruction. Indeed, it was rather a matter of regret with the Americans that they could not produce the thick armour plates which had been developed with so much trouble in this country. He did not know on what grounds it was asserted that the 'Dictator' was impenetrable to the guns of the 'Bellerophon.' There were guns of half the weight of those of the 'Bellerophon,' which, with smaller charges of powder, had penetrated 8 inches of armour: and if that were so, he could not understand why the guns of the 'Bellerophon' should not penetrate the six layers of 1 inch plates on the American Monitors. He thought Mr. Bourne would find cause, on further consideration, either to show how it was that the British 12-ton guns could not penetrate a succession of six separate 1 inch plates, or on that point admit he had made a slip. On the other hand, Mr. Reed was at a loss to know why it should be assumed that the 'Monitor's' guns would penetrate the 'Hercules.' The 'Hercules' target had actually resisted the shot of the English 22-ton gun, fired with 90 lbs. of powder, without a breach, except where two shots struck upon the same spot; and he was at a loss to understand why it was to be assumed that the 15-inch Rodman gun would so easily penetrate the sides of that vessel. He need only enforce what he said by inviting a comparison of the diagram of the side of the 'Monitor' with that of the 'Hercules.'

He had given some thought to the subject of turrets in sea-going Monitors, and he was not afraid of the low free-board in such ships, nor of doing without masts and sails in certain special vessels. He knew this was critical ground to go upon, especially the latter point; but the mention of it might excite considerations which would lead to solid conclusions. With regard to the former, his conviction was, that within proper limits, and associated with improvements in the construction of the hull with water-tight compartments, conforming with the structure and purposes of the hull and coal bunkers, &c., a low free board was a cause of safety, and not a cause of danger to a vessel; and, bearing in mind all he had said about the requirements of a ship, he felt that with a low free-board, a ship could be obtained which would not only be as safe as a broadside ship but safer. Nearly all the disasters to ships in the open ocean arose from this,—it occurred to the 'London' and many others—they took in water on the deck, the bulwarks retained the water there, and from thence, it went through the hatchways below. In the Monitor craft there was nothing to hold a single ton of water, and if there were no apertures for the entrance of the water to the lower parts of the ship, it would remain in safety. But he confessed if the low free-board of the American system, associated with the turret on deck, and apertures through the deck, were adopted in the Royal Navy, he should be in perpetual anxiety when the ships were afloat. For that reason, he had given consideration to the means by which it would be possible, in carrying great weights of armour, to economize the material so as to get the advantage of the low free-board of the Monitors, with raised apertures on the deck. If two turrets were placed on these ships—and he was not prepared to admit that a single turret was sufficient—these, with the funnels and ventilating shafts, all of which must be plated some considerable height above that low deck, would require a large amount of the weight of armour for the protection of those parts of the ship, as shown in Plate 9, Figs. 3 and 4. Instead of that, there might be, with no great increase in the weight of armour, an armoured breastwork about the base of all these various elements, as shown in Plate 9, Figs. 5 and 6, and all the apertures in the ship could be at once lifted 6, 8, or 10 feet higher above the water; and then, when the sea ran over the deck, as in the case of the Monitors, there was no danger of the water finding its way below, which had occasioned the loss of the 'Weehawken' and other ships of that class. His own conviction was that, with that arrangement, the advantages of the Monitors might be secured without their disadvantages. With regard to carrying masts and sails with this large amount of armour, his opinion was, that a ship with duplicate screws, quadruple engines, and an abundant supply of fuel, would be in a better position in stormy weather on a lee-shore than an iron-clad under canvas, in the event of accident to her machinery. He expected to hear great opposition to that view; but in these ponderously armoured vessels, it was far better to trust to good engines and plenty of fuel than to the winds.

Mr. J. D'A. Samuda, M.P., observed, that, as far as he understood the argument put forward in the Paper, it recommended that, whatever advantages might be obtained in the present plan of carrying guns below deck, and firing through port-holes, it was most desirable that the English fleet should be supplemented by a number of ships upon the turret system. On that general principle he quite concurred with the Author of the Paper, and he had even arrived at the conclusion, that the plan of carrying and fighting guns in turrets possessed a superior advantage to that in which the guns were carried below deck and fought from port-holes. Instead of making it a mere supplement, those who were charged with the administration of the navy should adopt this system, which was manifestly so much superior, and construct all new vessels upon it till some better plan could be shown. Agreeing, as he did, on that which appeared to be the cardinal point which the Paper put forward—and he believed it was gaining ground the more it was ventilated—he hoped it would shortly reach the official mind, and that the trouble of arguing this first cardinal point would be spared. He would therefore pass over that part of the question by simply saying this—that when, as at the present moment, the problem to be dealt with was, how to protect the sides of ships against shot, putting aside for the moment any particular description of vessel, it was clearly an advantage to have a vessel entirely covered with armour. The protection might be to that extent perfect in a vessel which fought its guns in turrets; but a vessel in which, at every interval of 15 feet along her sides, a large port-hole occurred, representing, in the aggregate, a considerable opening, through which the guns were fired, could not possibly be an entirely protected vessel.

SHIPS OF WAR.

Fig. 1.

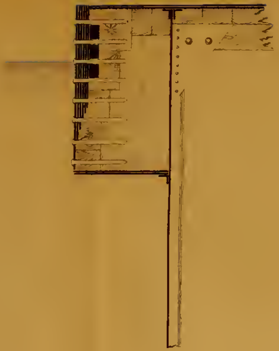


Fig. 2.

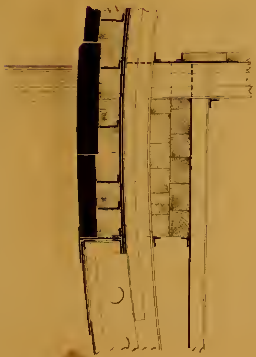


Fig. 3.

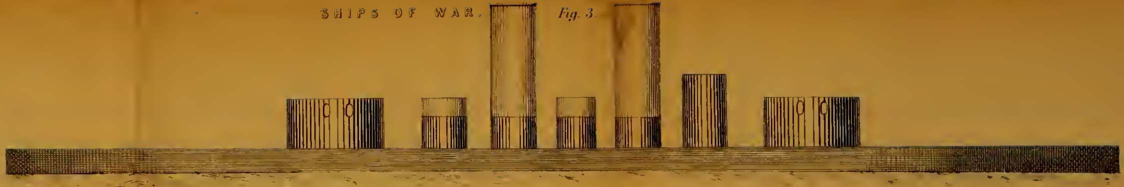


Fig. 4.

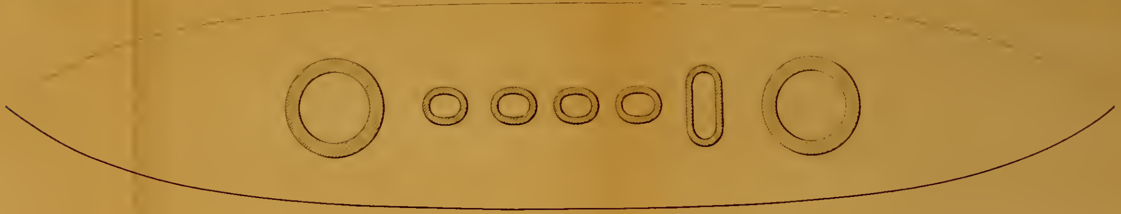


Fig. 5.

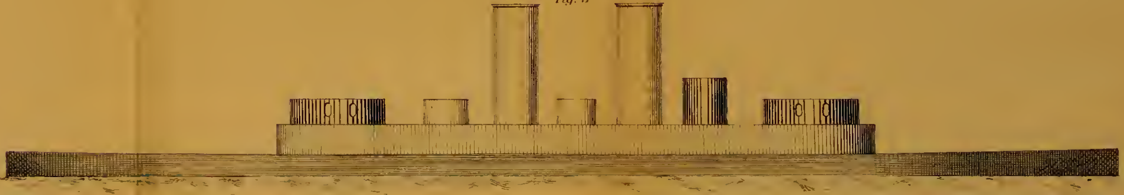
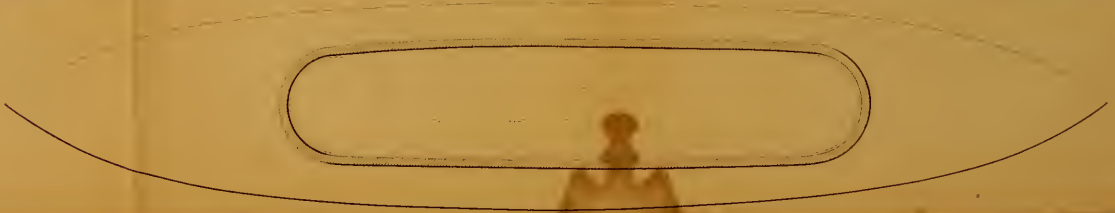


Fig 6.



Therefore, starting upon that first position, it would be seen that with the same amount of armour one vessel might be a protected one, the other would not be, and further, no matter what amount of armour was put upon it, it could not be made a wholly protected ship. Another point was this—the amount of range that could be got if two turrets were placed on a vessel. He concurred in the opinion, that two turrets were desirable when a vessel was capable of carrying them, and that a one-turret vessel was good, as far as it went, when it was not capable of carrying two; with two turrets an arc of the horizon of 150° out of 180° on each side of the ship was commanded; but in a port-hole vessel, the guns being carried in a central battery, the arc was restricted to something like 45° (except as regarded the bow and stern guns) instead of 150° as in the turrets. With this advantage, he thought that turret vessels were in every respect superior to port-hole ships. The point being then disposed of, that it was desirable to have a large number of turret vessels, the question resolved itself into a matter of detail, whether those vessels suggested in the Paper were the right vessels to have, or whether any other description of turret vessel would be better; and when he came to this point, he was sorry he could not express the same amount of agreement with the views of the Author. Instead of such a plan as had been suggested, of 314 feet length and 50 feet beam, with something under 2 feet of free-board, he would propose a sea-going vessel of less tonnage, with a length of 280 feet or 285 feet by 50 feet in breadth, with nearly 10 feet of free-board. Having gone closely into the question, he could state, with some degree of certainty, that such a vessel could be made of steel of the very best material producible at the present moment, and of such a scantling throughout, as to give a strength superior to that of any war-ship now afloat; and that it could be made to carry two turrets, and, with engines of 1,000 H.P., to steam 15 knots an hour. The difference between the vessel he referred to and that described in the Paper would be this; it would be a regular sea-going and sea-enduring ship, in which the crew could go to sea, and walk the deck for the benefit of fresh air, as in any ordinary vessel; whereas it was notorious that even if the speed was reduced to the ridiculously low rate at which the 'Monadnock' travelled—under 7 knots an hour, coming across with two tugs in company—the deck was not available, and the people on board were literally living in the ocean; it was a perfect sea prison, considering how long they had to remain on board, and the crew had no fresh air except that which was artificially supplied to them. All these things were great drawbacks to the system, but a still greater one, which he had not heard alluded to, was this: It was proposed, as an advantage to this vessel, that by having this extremely small free-board, and the extremely small depth of armour below, that, as a necessary consequence of the cutting down, much thicker armour might be used than on a vessel of the description he referred to; that in one case the armour was carried to a depth of something like 6 feet, whereas in what he described it would be something like 15 feet. It did not appear to have occurred to those who had put this forward as a great advantage, that long ranges would probably never decide naval actions in the present day any more than they did formerly. Perhaps the only instance that had taken place of a battle between armour-clads was the engagement off Lissa; and in that case, very far from availing themselves of long ranges, the battle was fought at as close quarters as any of the battles of Nelson; and it occurred to him, from what he had heard and read of that engagement, that the Austrian mode of mustering their forces, and going into action, as closely as possible resembled that adopted on several occasions by Nelson. If such a vessel as the 'Monitor' came into close quarters, it was true the sides might be protected by being so near the water, and presenting so small a target to the enemy; but a destructive fire could be opened upon the deck, which was so much below the level of that of the ordinary iron-clads. To meet this it was proposed to cover the deck with armour 2 inches thick; and as the deck was 50 feet wide, it followed, if the height of the deck enabled this to be dispensed with, it would go very far towards covering the additional 9 feet of side in his vessel with 6 or 8-inch armour. That was one reason why he considered there was a great failure in the selection of that class of vessel in preference to such a vessel as he had indicated. He would suggest that, if it was necessary to cover the sides with 18 inches of armour, it was necessary also to cover the deck with 18 inches; and that if 2 inches were sufficient for the deck, it was equally sufficient for the sides. He did not agree that it was necessary to put upon the sides of the vessel the amount of armour which had been suggested. He had heard it stated, that the 9-inch plate, such as was shown in Mr. Reed's diagram of the 'Hercules,' with its successive backings, was calculated to resist the power of the 22-ton gun at very short ranges. He had himself seen the 12-ton gun applied against every target that existed at the time these experiments were made, and he had seen it even fail to penetrate a 4½ inch plate and 18 inches of backing (which was an inferior description of armour to that used in some vessels in the present day), though only fired at a distance of 50 yards from the target. He had in one instance seen it penetrate a target with 5 inches of armour and a little less amount of backing, but whether penetrated or not in other cases, it was found to be as near as possible the measure at which the shot could get through the armour; but when 5-inch armour was talked of, that was very small in proportion to the thickness of armour on the 'Dictator,' which might be taken in round numbers at 12 inches; but if it was put in plates separately, the 12 inches of armour formed of a series of 1-inch plates really represented a smaller amount of armour than 4-inch armour put on in one solid plate. The resistance of those plates had been found to be as nearly as possible as the square of the depth, while that of the series of plates was as the depth only. Therefore a 4-inch solid plate would have a resistance equal to sixteen 1-inch plates riveted together. If 9-inch armour had resisted a gun so formidable that but one of them had been able to be made, and which was now burst and gone, did it not appear a most Quixotic arrangement to think of loading the sides of ships of war with 12 or 15 inches of armour? He had no doubt the Americans would find that solid plates were more efficient armour than a series of thin plates riveted together, which was an armour not to be relied upon; and they would probably change their present views on that subject, and when they did so they would probably change their ships. But while there were no guns that could destroy a 9-inch plate, it was of no use loading ships with an unnecessary weight of armour. His own view was, that the limit of the thickness of armour was already attained in this country, and that the general tendency would be certainly not to increase, but probably in some degree to diminish the thickness of the armour. He did not consider penetration of armour by solid shot was a matter greatly to be dreaded. It was only necessary to make the ships with their defensive powers equal to stand up against the offensive attack of the guns of the present day, in the same way as the famous wooden ships of old had to stand up against the inferior guns of their day. In the old wooden ships of the days of Nelson there never was a ship that could not be penetrated with shot. They were damaged, and they stood up for a certain time; but since the introduction of the more formidable shell into naval munitions, he agreed it was necessary that the armour of the present day should be a protection against it. But had any shell ever been produced which had been able to penetrate 5-inch armour? He could answer there had not; and he could go further, and say, as far as could be ascertained with the present amount of knowledge, there was no possibility of a shell being made to penetrate solid 5-inch armour. The force of impact was so strong in boring through the armour plate, that it was long ago found impossible to use any fuse on the shell. To get over that difficulty, it was ingeniously suggested, that the shell should be made of cast steel, that the powder should be put inside the shell without any fuse, and that the amount of heat evolved in the difficulty of penetration should serve to ignite the powder and burst the shell; but it never was considered, that, if a sufficient amount of resistance were obtained to give that heat which would ignite the powder, there would be engendered at the same time a sufficient amount of disturbance of the material of the shell to break it to pieces in the effort to get through the plate. In no case had he seen an instance of a shell going through the armour plate into the ship whole and undisturbed, and, by means of the heat evolved in the action of getting through, become exploded. It was nothing more than a missile cutting a clean hole and falling, as he had seen it do, into a chamber made to receive it, doing no damage to the chamber, though it was only formed of 3-inch planks. If, therefore, the armour was capable of resisting shell, he considered everything necessary was attained. It need not be objected that the armour would not keep out solid shot; for when it went through it made, with the best armour plates, a clean hole corresponding in size somewhat to the shot, and by no means a difficult one to plug. His opinion was, with the present knowledge of guns and armour, nothing was necessary beyond 6 inches of armour, or, in the very excess of circumstances, 8 inches of armour; and such a vessel as he had described of 3,500 tons, worked by double screws and 1,000 H.P., could carry 8 inches of armour over the whole surface of the skin of the vessel; could carry two turrets; and two of the largest guns, or four 12-ton guns, and would be altogether a more useful vessel for sea-going purposes than the Monitors. Therefore, though the suggestion to recruit the navy with a large number of turret vessels might be approved, it was most undesirable to come to the conclusion that the vessel of Ericsson, as described in the Paper, was by any means the right thing to adopt. Then again, as far as the plan of the turrets themselves were concerned, there might in some cases be advantages in using Ericsson's plan over Captain Coles' plan, though he thought the latter would generally be found preferable.

With respect to the form of bow for the purpose of ramming, it was suggested as a difficult thing to ram the Monitors, and the bow of the 'Bellerophon' was put forward to show it would not ram those vessels. No doubt the Author thought he had selected an appropriate instance to show the best description of bow that might be brought against such vessels, and the resistance they would offer. But he considered the bow of the 'Bellerophon' to be a great mistake, and a bow which he hoped would never be repeated in the navy; no doubt, it was designed with the best intention of ramming under water, when it was not known what the result of that description of bow would be. He made one of these bows some time ago, for a foreign government, because they held the opinion that its ramming qualities were very good, and that it had no bad qualities; but when tried, he found it entailed a loss of three-quarters of a knot an hour in the speed; and he had no doubt a large amount of the failure of the 'Bellerophon' in speed performance below the 'Warrior,' 'Achilles,' 'Minotaur,' and 'Agincourt,' was due to the decreased mechanical effect obtained by the engines, consequent on having such a bow to drive through the water. When he drove this bow fast, the water was forced in at the hawse pipes; and he could not believe it possessed advantages in ramming qualities beyond those possessed by ordinary bows, slightly projecting at the water-line, which would have a tendency to bear down a vessel with low sides, to run completely over her, and to sink her altogether. Sufficient, he hoped, had been shown, to induce a general belief, that the advantages of the turret system would be recognized, and he trusted also to induce the authorities to pause before they accepted, as a necessity, that which had been cleverly done on the other side of the water, but which he looked upon as a mere makeshift, compared with such a vessel as could be suggested, having the same offensive powers as those vessels, and superior sea-going qualities and comfort for the crews on board.

Mr. F. J. Bramwell remarked, as to the actual results obtained in the penetration of shot, that he understood Mr. Reed to state, that 8½-inch armour plate had only twice been penetrated, and then with two shots from a 22-ton gun striking on the same spot.

Mr. Reed said, what he stated was, that the 'Hercules' target was never penetrated.

Mr. Bramwell: How far that might modify the case he could not say; but he had been present when a target of 8½-inch solid plate, with 18 inches of teak at the back, an iron skin next, and then strong iron ribs, had been penetrated with the greatest ease by a 9-inch Palliser shell, fired from a 12-ton gun, at a distance of 200 yards, with a velocity of shot of a quarter of a mile per second at the moment of impact. It was well to state this, because he thought arguments had been raised upon false premises, and that there was some misconception when it was asserted that 6-inch to 8-inch armour plate was the outside of what was necessary in the way of protection against shot. A 22-ton gun was not necessary to force that. He believed it was quite certain that a 7-inch shot entered a 4½-inch plate, and went through it out to sea, with only a 12 lb. charge. It would not, therefore, do to argue for the restriction of armour plating, when missiles, different to those previously introduced into the service, were employed. By the use of pointed shot, which concentrated the whole energy of the shot upon a very small space, made of the unyielding material of chilled cast iron, which enabled the whole energy to be concentrated upon the point, without altering the form of the point, he believed plates, hitherto thought to be impermeable to shot, would be found capable of penetration with a considerable amount of ease; therefore, in a discussion of this subject, what had been done within the last six months should be borne in mind. It had been argued, with regard to the upper deck of the Monitor, that, taking the width of the deck at 50 feet, the sides might be plated with 5-inch plates for a height of 25 feet, and no more iron be used than in the present covering of the deck. Captain Noble produced before the British Association a formula of the effect of shot, which showed the diminished effect, according to the angle of inclination at which the shot struck the object, and also that a shot, which would do but little damage when fired from an elevation upon the deck, would do a great deal of mischief when applied to the vertical side of a ship. On the subject of the Paper, he would say that ships with so small an amount of free-board, reminded him of the instrument called the hydrometer, by which the specific gravity of a liquid was measured by the degree of immersion of a metallic ball attached to a slender stem beneath it. When once it was put in motion its undulations lasted for some time, and very little force would set it moving up and down. Looking at this vessel floating within 16 inches of the surface, he could not help thinking that when she got between two waves, rising one moment and falling the next, it tested on a large scale the specific gravity of salt water, but in a manner extremely inconvenient to those who were in the vessel. He was therefore inclined, on these grounds, to favour the suggestion of 10 feet of free-board instead of only 16 inches.

Mr. J. A. Longridge confessed that some of the previous statements had taken him by surprise. It had been stated that the effects of shell fired against armour-plated ships need not be feared, inasmuch as the force of the impact against the iron sides would break up the shell, and render it harmless for penetration, or for the other destructive qualities appertaining to that missile. He trusted some official information might be given on that point. With regard to other matters, more especially the subject of the Paper, knowing but little of ship-building, he would say nothing; but he would go so far as to say he believed the art of gun-making was at present only in its infancy, and that a really large gun had never yet been made. To talk of the American 15-inch gun was trifling. The Americans had guns of large calibre, it was true, but the velocity of the projectile was so low as to make the effects inferior to those of the British 7-inch gun. The great thing was to see, in the first instance, what artillery would do, and on that point he did not think such progress had been made in the last five or six years as might have been expected. He hoped, before the close of the session, to bring that subject before the Institution, which he trusted would be the means of eliciting what British guns had done; because he must say all who were not in an official position were much in the dark on that matter, and were almost entirely dependent for information upon news-paper reports, in which the most contradictory statements were sometimes made. From what he had gathered, he came to the conclusion that up to the present time there was no reliable 9-inch gun in the service, but he thought that a 9-inch or even a 12-inch gun was by no means the utmost limit which would be ultimately attained. If an opportunity was afforded for raising that discussion, he hoped to hear more of what had been done during the last six years; and when that was ascertained it would be time to discuss the question of defences. He thought guns might be produced which would render all offensive operations against the English coasts impracticable, and if proper attention were paid to the defence of their shores, two nations like England and America might set all other nations at defiance.

Mr. Robert Mallet wished to correct a matter of fact with regard to the penetration of shells. He had been present at the experiments of the 24th October, 1866, at Shoeburyness, and so far from its being the fact that shells fired from the 9-inch gun did not penetrate the heavy armour they were fired at, the empty shells passed through plating and backing with greater facility than solid shot, the reason of this being the higher velocity which the comparatively light shells acquired. Both shot and shells broke up more or less in passing. One live shell passed through and burst behind, and one fragment knocked off a corner of granite of very large size. With the elongated shell there was a difficulty in getting space for a large bursting charge, and to effect that the shell was often made so thin that it broke from the force of the impact. The artillerist found himself between Scylla and Charybdis, either to limit the bursting charge, or to make the shell so thin, that it broke on striking the object against which it was directed. The result of the late experiments was, that though the white cast-iron or, so called, Palliser live shells broke up, and exploded in the body of the target, yet the inertia of the rear part of the shell afforded sufficient fulcrum to the bursting charge to drive the fragments of the front part through the target, forming a destructive mass of heavy *mitraille* between decks, with a large cone of dispersion. He thought, therefore, it must be conceded that armour-plates were necessary to resist shell as well as shot. He would ask those of more experience than he had, what it was thought would be the effect upon armour-plates of these heavy American shot, fired at comparatively low velocities. General Cavalli, of the Italian service, had made some important investigations for his Government on the subject of armour plates, which were not as yet published; and he thought he might say that distinguished officer had advanced further, theoretically, on the subject than any other man living. He had treated of the recoil of armour plate, with elastic backing, struck under various conditions. One result of the investigation, to which he referred, was to show, that if very heavy shot were fired with considerable, but what must still be viewed as low, velocities at the sides of a ship covered with heavy armour-plates, though such shot might not penetrate, or bend, or break them, yet a succession of such shots would jump the plates clean off; and if large armour plates were knocked off in that way, what was to prevent the common 68-pounder going through the vessel afterwards?

Mr. T. A. Rochussen, as an old Dutch sailor, looked with great discomfort upon the craft under consideration, and he quite concurred with Mr. Scott Russell's definition, that it was only a floating gun-carriage. It might have been added, it was a carriage for a gun which did not exist,—a gun which would not be made—but which, if made, would not suit the present system of artillery practice; consequently it might be concluded that it was a gun which, if the Americans had the forging power of Europe, they would not have made, and then the gun-carriage would not have been in existence. Looking at the design, it occurred to him that there was very little stowage room either for ammunition or for men; and it was deficient in representing that which a ship of war should be—viz., the concentration of a small military power. A vessel of this class was necessarily limited in its capacity for carrying fuel, and must be dependent upon coal depôts near at hand; and the living power of the crew was measured by the supply of air to be obtained from the tube through the deck; and the discomfort of those on board which would follow any derangement of the ventilating machinery, especially in a hot climate, might be imagined. Supposing a vessel like that took fire, and it was necessary to throw the powder overboard, it would be hardly possible to get the powder on deck through such a turret as that, which would in fact have become the throat of a blast furnace. It was true two vessels of this description had made long passages, one having reached the Pacific, and the other having crossed the Atlantic; but they had done so convoyed by vessels of a totally different class. Supposing, however, they had been unaccompanied by other vessels, and supposing they had been driven before a gale of wind, with long rolling waves, such as were met with off Cape Horn and the Cape of Good Hope, he apprehended their position would be such as Mr. Bramwell had so well described. By the increasing initial velocity of the waves, and the absence of buoyancy in the hulls, they would gain a 'bobbing' motion, and finally be pooped altogether. Reference had been made to the probable effects of a collision, by ramming, between the 'Dictator' and the 'Bellerophon,' and also as to the alleged error in the form of the bow of the latter vessel. Now it occurred to him that, if European iron-clads had habitually to fight vessels of the Monitor class, the bows, instead of being like the breast of the swan, should resemble the snout of the alligator, when a very powerful engine of destruction would be at once produced. He thought there were many disadvantages attending the low free-board of vessels of this description, inasmuch as it prevented them carrying boats and canvas, and exposed them to plunging fire and boarding attack; and he conceived a vessel of this description was always in danger of meeting a larger vessel of the European type, which might resort to the Chinese mode of warfare, and fire stink-pots down its turrets and air-pipes. At the same time, vessels of this kind would form a valuable adjunct to the navy of a nation wishing to rank as a first-rate power. Looking at what had been done in America, the Northern States of that country had kept the monopoly of power in the Mississippi because their vessels were invulnerable. It was not so much sailing-power in that case as invulnerability. If this country were at war with France, nothing could be more efficient, for preventing the egress of transport-ships from Cherbourg, than two or three of these vessels placed off Alderney. They might be kept at bay by the large armour-clads of the French navy, but they would be able to destroy every transport that attempted to leave the port. Then again, with a few of these Monitors on the Rhine, or the Scheldt, no hostile army would be able to hold the banks of those rivers. Consequently, though these vessels could not be held up as a pattern of sea-going ships, they might very properly be an adjunct to the navy of a nation which showed its flag all over the world.

Mr. ZERAH COLBURN said, remarks having been made as to the initial velocity of the American guns, he would take the opportunity of stating, from information he had received of American experiments upon armour and fortification, that in many cases, with 440 lbs. shot and 60 lbs. charge of powder, the initial velocity was 1,300 feet per second; and, from calculations he had made, he found that the dynamic value of each pound of powder averaged 200,000 foot pounds. That, he believed, was a greater dynamic power than had been obtained from powder burnt in the English guns. The difference was not in the strength of the powder, but in the ordnance itself. Powder-gas expanded in the bore of the cannon, and in a much larger chamber in the 22-ton gun, than in the 12-ton gun. The greater expansion of the powder-gas would seem to account for the superior dynamic value in the American service. He hoped some authoritative statement would be made with respect to the alleged low initial velocity of the American ordnance, as he thought it would not be found to be so low as many supposed.

Mr. VIGNOLES observed, that a great point for consideration was as to the perfect consumption of the powder at the moment of the discharge.

Vice-Admiral ELLIOT said this subject embraced so many points for discussion that it was impossible to enter into them all; and as many had already been treated, he would endeavour to take up those which had been omitted. He thought the naval architect was generally unfairly treated when asked to design a Ship of War, because he had not laid before him by the practical Engineer, or rather by the sailor, the conditions upon which he was to design the vessel. He thought the naval officer ought to have a voice in the matter, and an important one; and he also thought, if the sailor had been side by side with the naval architect, and had assisted in producing Ships of War, and if it had not been left to the theorist to advance sailoring ideas, the iron-clad fleet would have been more efficient than was the case at present.

In the first place he thought there ought to be a starting-point; and before he asked the naval architect to design a ship, he should tell him exactly what was required. If he said, "Build me a ship as a man-of-war," and left him to devise what that ship should be, he did not think it was fair to make him responsible for the defects and the absence of qualities in her which would be complained of by those who had to work and fight her. He had, therefore, endeavoured, if possible, to get some starting-point; for he saw ships of different kinds, and if he asked for what purpose a particular ship was intended, he could not find out whether it was intended to do duty as a line-of-battle ship, a frigate, or a corvette. He considered, from what he saw, that generally speaking, the attempt was made to do all sorts of things in one vessel; and he was certain the result would be that no ship would be suited for any special service. He was strongly of opinion that there should be in the Royal Navy, as formerly, ships classified and suited for special purposes. A line-of-battle ship was a war vessel which was supposed to sail, as her name implied, in the line of battle, and should be specially designed for that purpose, to secure victory to the great fleet by which alone the mastery of the sea could be maintained. He believed, in a future war, the only way to remain masters of the sea was by a strong fleet of line-of-battle ships; whether one-deckers or two-deckers, they should constitute the fleet for line-of-battle purposes. If they had not great speed, it was true they might not catch an enemy's fleet; but if they possessed great fighting powers, they would command the sea. Therefore, he said to the naval architect, he wanted a line-of-battle ship first of all, and he laid down certain conditions; and he asked that those conditions should be complied with as far as possible.

First in importance he wanted the line-of-battle ship to be a fighting ship. He would sacrifice everything else for that, because he wanted to win the battle whenever it occurred; and if the enemy's fleet ran away because he had not speed—and in this class of vessel he had put "steaming" No. 5 in the following Table of qualifications—

CLASSIFICATION for SHIPS OF WAR, and Qualification for each Class in numerical order.

LINER.	FRIGATE.	CORVETTE.
1. Fighting.	1. Steaming.	1. Steaming.
2. Turning.	2. Fighting.	2. Sea-going.
3. Sea-going.	3. Sea-going.	3. Stowage.
4. Sailing.	4. Stowage.	4. Draught.
5. Steaming.	5. Sailing.	5. Sailing.
6. Stowage.	6. Turning.	6. Turning.
7. Draught.	7. Draught.	7. Fighting.

if they ran away, he was still master of the sea. He had, therefore, put fighting qualities first in that class. He wanted the ship to turn quickly, because the manœuvring of a number of ships in a great sea-fight was an element of success, which would greatly decide the result of that battle. Therefore turning came second. He would give to that ship the thickest sides, and put into her the heaviest guns. He would make her deep in the water if it was desired, because speed was not the great necessity. He would thus make her capable of turning quickly. She must be a sea-going vessel, because she would have to remain at sea. She must carry canvas to keep her in line, and enable her to cruise without steam, for if she were dependent entirely upon steam, she could not do that for any length of time. Therefore sea-going and sailing came third and fourth. He took stowage No. 6 on the list, because she could be accompanied by transports carrying coal and provisions to a certain extent. The sailing quality was necessary for the purpose of keeping the fleet together when at sea, and that was by no means an easy thing to do. He was of opinion that a line-of-battle fleet should not be left entirely to steam power, inasmuch as thereby the utility and safety of the fleet were to a great extent sacrificed. He, therefore, put sailing before steaming, then stowage, and then draught of water. The naval architect would say, "If I may go down deep into the water, I can have a short ship, which will turn quickly, and I can put thicker armour-plate upon a smaller surface of sides."

He then came to the frigate class of vessel; and he would say that class was as much required now as ever it was, to follow up an enemy's fleet, and keep a watch upon it. For this purpose a frigate should be able to escape from the enemy, and have the power of defending herself in single action, if necessary. Therefore he put "steaming" as the first element, because he required her to keep close to a strong power, and to be able to get away if pursued. Next to "steaming" he put fighting, afterwards "stowage," and then "sailing," because, as she was not to go in line-of-battle, he preferred that her sailing qualities should be reduced, and a sacrifice must be made of some quality for the sake of speed of steaming. He put turning lower on the list, because he did not care about her being a quick-turning ship. She could do all the duties of a frigate in serving as the "eye" of the fleet; and tonnage he allowed to be unlimited, because he believed these qualities of great steaming power and fighting power could not be combined, without great tonnage. He said then the frigate would be a larger ship than the line-of-battle ship, because speed was the great desideratum in the former class of vessel. While the 'Warrior' class acted as the frigates, he would take the 'Bellerophon' as the line-of-battle ship, except that he would not put in her 6000 H.P., as half that power would enable her to be made a better fighting ship.

He next came to the corvette class, which he would make the fastest vessels possible; the object of this class being the protection of merchantmen on the sea, to destroy the trade of the enemy, and to put down privateering. For this purpose he sacrificed everything for speed in a corvette, so as to enable her to catch anything she came across, and to escape from any hostile man-of-war she fell in with. She must be sea-going, her draught must be limited, her sailing powers also limited, and turning was not much required; fighting qualities he placed last on the list. He would put no armour whatever on this class of ship; and he believed if the Admiralty had fifty or a hundred of these ships to send to sea it would be a better security for peace than fifty line-of-battle ships; because what frequently led to war was the temptation to prey upon another nation's commerce; and if it were known that Great Britain had one hundred of these ships ready to send out, foreign nations would be disinclined to risk the chances of war.

He had drawn out this classification of ships in a hasty way, to guide the naval architect in designing a Ship of War; after that he would leave the naval architect to act upon his own resources for fulfilling the required conditions; and if the elements of those ships were not properly placed, that would be the concern of the naval architect. Having given certain limits, he asked him to design the ship, and he should not attempt, except in practical matters, to interfere.

Having thus expressed his views as to what Ships of War should be with regard to classification, he would now refer to the Paper. The proposition was, that vessels of the Monitor class should be built, to a considerable extent, to accompany the broadside ships. He wished it had been explained how broadside vessels might be useful and even necessary, when the Author said he could imagine cases where they might almost be indispensable. Admiral Elliot did not understand that, as he could not see wherein broadside ships were indispensable. He thought the Author must have meant that built-up ships, whether broadside or turret, were indispensable. He should be sorry to see anything like the Monitors introduced into the navy as sea-going vessels. He was certain that, as a habitation, they would not answer; and when he said that, he was prepared to propose how to make a vessel equally formidable with the Monitor, and having all the comforts and conveniences of the old type of vessels. He would merely build upon the Monitor a habitation; and if in a battle all that was knocked away, the Monitor itself was still left underneath. To do that he must give the naval architect more displacement, to provide for the additional weight which the vessel would have to carry. In that way he got a more expensive Monitor, but he, at the same time, carried out the two principles in one ship, instead of having two separate fleets, one of broadsides and the other of Monitors. He thought the two could be put into one with a low free-board of armour-plating, but with a high free-board for the habitation, and on the top of that the turrets, and then with a spar deck, bulwarks might be dispensed with.

He was an advocate for turrets, and had been opposed to broadsides ever since turrets were introduced. He preferred Captain Coles' turret because it rested upon a more solid base; and as to its not being possible to keep the water out, he had seen it tried, and he should be as little alarmed at a drop of water going through the deck of the turret as at a slate being loose on the top of his house. He therefore proposed to adopt Captain Coles' turrets; and he would explain to the naval architect, that he would be satisfied for a fighting ship if he defended the vital parts of the vessel, and he would name to him the thickness of armour-plating, and the other conditions required, in numerical order.

With regard to the thickness of armour-plating, that was another question, on which he would be glad to see a decision arrived at; but that could only be done on such information as had been already obtained. He thought the limits of the gun might be calculated, and when a foot thickness of iron was reached, he thought that would be as much as would ever be necessary. As a member of the Royal Commission on National Defences appointed some years ago, he endeavoured to come to some decision as to the limits of guns and plating; he thought the general conclusion arrived at was this—that guns might be increased in size till ultimately the expansive force of the powder would be so great as to destroy the metal inside. Having resorted to steel tubes, he thought it would be possible to increase the toughness of the armour plates as well as the toughness of the inside of the gun, and to keep pace with the gun; and thus in proportion as the power of the guns was increased, so would the defensive powers of the armour-plate be increased; and in that case a foot thickness of armour would probably be the utmost to which it would be necessary to go for naval warfare.

The Chief Constructor of the Navy had made a proposition for an improved Monitor (Plate 9, Figs. 3—6); but he (Admiral Elliot) ventured to think it was not an improvement. Monitors permitted the sea to wash across them; but in the design submitted by Mr. Reed, the sea would strike the central sea wall with great force, and come inboard over the top. He did not consider that was an improvement, and he should be sorry if the future ships of the navy were to be of that class, without masts or sails, and dependent entirely upon steam power. He had had some experience; and he believed, with a fleet of twenty vessels, scarcely forty-eight hours passed without some mishap occurring to the machinery of one of the vessels, which obliged it to stop; and when that occurred in narrow waters, or upon a lee-shore, the results might be of a very disastrous nature; but he had no fears that the present Admiralty would sanction such a navy being constructed, for seagoing purposes, as the Chief Constructor proposed.

During the last year, two inventions had arrived at a success which would aid the naval architect immensely: one was the principle of hydraulic propulsion, which he was certain was a success; and the other the new heating agent which had been discovered in shale oil. If by that means a saving of two-thirds of the coal ordinarily carried was effected, it would be possible either to reduce the vessel's displacement to that extent, or to increase the thickness of the armour in proportion to the reduced weight of fuel to be carried. The application of the hydraulic propeller had at present been confined to the gunboat type only. He was in no way answerable for the character of the 'Waterwitch' as a gunboat, his responsibility was confined to the motive power. Certain gunboats had been built which manifestly did not answer, and he thought an armour-plated gunboat would not be fit to go to sea. He had placed on the table a model of a gunboat furnished with a strong breastwork of iron, behind which the gun could be fought. The armour-plating was 1 foot thick, to a depth of 3 feet below the water, with a deck at that level of 3-inch iron, and compartments up to the water line filled with cork. Both ends were intended to be alike, and she was proposed to be fitted with hydraulic motive power. The approach to an enemy's vessel was made stern foremost, as presenting the greatest facility for working the gun, from its freedom from the obstructions of anchors and gearing at the bows, whilst there was nothing of the sort over the stern. These boats might be rigged like a brig or a barque, with tripod mast for the mizen-mast. As he had said, she advanced by the stern, and if chase was given, she went away in the ordinary direction; whereas, in the case of gunboats bullying large ships, they were obliged to turn round before they could run away, and they were in risk of being caught before they had time to get into shoal water; they then presented their stern, which was their weak point, to the enemy's guns; whereas in his plan the stern, which was strongly protected, was always pointed towards the enemy. He believed in that way he would be able to do a large ship a great amount of mischief, and escape with very little harm to the gunboat.

With regard to the turret principle, he looked upon it as one of its greatest elements of success, that it was of no consequence how such a ship was knocking about; the gun could be trained fore and aft, and easily re-loaded. He knew the great difficulty there was in loading guns in a broadside ship in a heavy sea, with heavy shot to be lifted and weighty appliances for the loading; whereas in the turret the gun could be trained fore and aft, or abeam, in fifteen seconds, and the port-holes would only be exposed whilst in the act of firing. The port-holes of the broadsides were tempting marks for an enemy's musketry, and a few men with rifles would be able to prevent a gun being worked at close quarters. These were advantages which spoke in favour of the turret system with a voice that ought to be heard.

It had been stated that an opportunity would probably be afforded, on a future occasion, of discussing the question of the motive power of ships; and when that took place, he hoped to be in a position to bring forward the most conclusive testimony of the merits and success of the hydraulic motive power. He had challenged all who chose to do so to come and see it, and try it in the 'Nautilus;' and it had been tried in the 'Waterwitch,' a sister ship to the 'Vixen' and the 'Viper.' The only changes made were, fitting the rudder at the other end, and altering the stern to the shape of the fore-body; there was the same displacement and the same amount of boiler-power. The speed was greater in the 'Waterwitch,' both at light and deep water-line, than in the sister vessels with their screws; notwithstanding, it was stated that the speed was not considered commensurate with the power exerted. But he maintained, that so far it had proved successful, and that as the power was increased, so would the advantages of the system be increased, because the friction would be comparatively less as the size of the pipes was increased. The friction of the two engines was the same; and there was the slip of the screw in the one case, against the friction of the water in the pipes in the other. He was very delighted to find that the same results were produced whether the nozzle was under water or above it. The nozzle might, in fact, be placed at any depth, and without projecting at all from the ship's side;

which gave, in fact, a perfect sailing-ship, combined with a motive power which could be used when liked. He thought the system would be found a highly economical one in a commercial point of view, and for naval purposes, would possess enormous advantages, inasmuch as it gave the power of ramming both ways. He believed future battles at sea would be decided by ramming more than by guns; therefore, when the occasion arrived, he trusted he should be able to bring this subject more prominently before the Institution.

Captain SCOTT, R.N., remarked, that the Author of the Paper laid it down as a first principle that defence was the essential point to be regarded—that defence consisting in the thickness of the armour-plating. Captain Scott thought it was wrong to go to defence first; for when a ship was furnished with the highest amount of offensive power possible, she really would be best defended. The Monitors, in his opinion, resembled the tortoise; it could not be got at when closed in its shell, but it was itself helpless, and could not damage anything else. At the siege of Charleston what did the Monitors do? One of them got aground, on which a fire was opened upon her from the fort, and the whole fleet of Monitors were not able to render her any assistance, as they could only fire one round in five minutes, and that in a very irregular manner. The shot were not properly directed against the embrasures of the enemy, because as the aperture of the turrets came round, the guns from the fort let fly, and checked the Monitors' fire. The broadside ship 'New Ironsides' was signalled to go in to the aid of the Monitors, and in ten minutes she silenced the fire of the batteries, and thus the purblind monster, to use an American designation, was enabled to get away. At Mobile Bay other instances were afforded of the inefficiency of mere armoured defence. One was the case of wooden vessels driving at and overpowering an iron-clad of this sort, which could neither fight her guns nor fly. She was just like a tortoise: she could not use her guns, but her defensive shell remained complete. Again, in Mobile Bay, one of the leading Monitors went in, but the succeeding one did not follow after the first—the 'Tecumseh' getting a torpedo underneath her, and turning over like a turtle, and then the wooden flag-ship 'Hartford' led the way. Then again what was done in the action of Lissa? The Austrian flag-ship, acting offensively, rammed the 'Ré d'Italia,' and sent her to the bottom. In this battle the Austrian vessels took the offensive part, and victory was the sure result. England had won all her naval battles by her attacking, and not by her defensive power; and the sudden attacks of boarding parties had often turned the scale in her favour. He had heard officers say, only give English sailors broomsticks, and with a rush they would carry everything before them. He was satisfied it would be a dark day for England when, instead of seeking out the enemy, her sailors would be content to cower behind walls, whether of iron or of stone. With regard to the rapidity of fire of the Monitors, he believed the best instances, under the most favourable circumstances, were one round in about 2½ minutes. In the 'Royal Sovereign,' after a year's practice in the turret, the best firing was ten rounds in 13 minutes 21 seconds, or 1⅓rd minute between each round; while in the 'Minotaur,' a broadside ship, ten rounds were fired in 4 minutes 48 seconds after the men had been practised at the guns for only a few days. The effect of such rapid firing would be to keep down the enemy's fire altogether, and when that was done he was really conquered. The Author stated that the broadside system was one of diffusion, and the Monitor system one of concentration. Was it that the Monitors' concentration consisted in delaying their fire, while the diffusion of the broadsides resulted in keeping it up, actually firing five rounds to the Monitor's one? That was the actual rate of firing of the two vessels. The Monitors only fired one round in 2½ minutes, while the broadsides could keep up, regularly training the gun each time, ten rounds in less than five minutes. And further, as to the question of diffusion and concentration, after seeing heavy guns fired, his views were, that if there was only one turret in the centre of the ship, as Captain Ericsson preferred, it would be shrouded all round by powder-smoke, and the crew would have no opportunity of seeing the enemy, or even as far as the length of their own vessel, and under such circumstances ships in a squadron were just as likely to fire into their neighbour as into the enemy. For that reason he considered it necessary to have heavy guns at each end of the ship as well as in the centre, and that was a conclusion at which almost every officer of the Channel squadron he had spoken to had arrived at.

The Author complained of a great want of innovation, and he appeared to think that was more especially the case at the present time. Captain Scott thought what had already been arrived at was only reached after great study. The present age was one of progress, but it was not always wise to depart from what had formerly been worked out so well. The plan proposed by Admiral Elliot was an attempt to combine in a vessel carrying heavy guns all the advantages of light living decks at each end. That vessel was designed in 1862 to carry 20-ton guns, and was officially put forward before there was a single heavy gun afloat in a Monitor; and from that time the broadside system had been worked upon till it now appeared in other forms. There were projections of rounded form upon each broadside mounting powerful guns, such as the 'Hercules' might be supposed to carry. There was a succession of these projections—say three in the central portion of a frigate carrying 18-ton or 20-ton guns. The advantage of having such a battery as that was that the broadside guns could be trained within

13° of the keel in all these projections together. If there were three guns on each side, then all three could be trained through an arc of 154° on the broadside. Such a vessel had room for light living decks at the extremities, with the further advantage of being able to fire three broadsides per minute, laying with the bow or stern towards the enemy. Thus while a vessel of the Monitor class would only be able to fire her 15-inch gun once in 2½ minutes, twenty rounds could be fired from this vessel in the same time, taking the practice of the Monitors as a standard.

He did not say that the Monitor turret system of working guns was not capable of being improved, for it was a new development as compared with the broadside; but the broadside must not be too hastily given up. It might be thought the fire from the gun's discharge at a port in one of these rounded projections would go into a port in the next projection when the guns were trained so near the line of keel; but in this system the ports not occupied by the guns could be shut up, and the others dropped after firing. Then, by having a small scuttle-hole, the gun might be reloaded in comparative security.

As to traversing the guns, great difficulty had been experienced in turning them from one position to another. A plan for effecting this, by means of a small turn-table at the rear of the gun, would probably be carried out in the 'Hercules.' The end of the gun would be brought over the turn-table, on which the gun would be swung round to the next port, which could be accomplished in about ten seconds; the gun was breached upon the slide, which made the operation simple.

With respect to gunnery practice, he agreed that to any one who did not follow the matter up closely the whole thing was a puzzle. It was often stated, as he conceived absurdly, that a shot fired at a distance of 200 yards, represented the effects of a shot fired at a distance of 2,000 or 3,000 yards. Now the trajectory of the shot falling upon an object at the latter distances would be such, that it would strike at a considerable angle with the horizontal line; but when fired at 200 yards it would strike nearly straight. But then there was the question of the spin of the shot. The reason why some rifled shot had not much penetration at a great distance was because the spin of the projectile was too much reduced. The Whitworth gun, which had a sharp spiral, when fired at a distance, was found to have greater penetration than the Armstrong, because the initial velocity of the shot from the latter was only about one-half that of the former; and the longer the range, the greater would be the advantage in this respect. At long range there must be more spin than the present service-gun gave its projectiles.

With regard to Mr. Reed's vessel, the important question was, whether it would be good for fighting purposes or not? He should be glad to see a ship of that class built, because he thought that, with independent engines, and the other appliances suggested, it would be a very formidable and powerful vessel. But there seemed to him to be a want of a living deck; by tacking on to it a light living deck, and bringing up the height of the bulwarks, he thought she would make a very effective vessel; although by bringing up a light deck in that way, the power of depressing the central guns would be lost. With a large gun that might not be of consequence, but with small guns it would be. It was, therefore, necessary to neutralise that disadvantage by having light guns at the side, which could be fired at a great angle of depression, to supplement the other guns. At present there were no broadside guns that could be fired at a greater depression than about 5°; so that if a Monitor got alongside she could not be hit. It was essential to be able to fire downwards; and though the Armstrong guns were being taken from the vessels, he believed the breech-loading Armstrong would now be of use in firing through the deck of such craft as the Monitors under consideration, or in sinking torpedo vessels. With a specially-constructed gun-carriage, 25° of depression and 25° of elevation could be obtained without difficulty.

Captain Hamilton stated, that the remarks he purposed making would have reference to that portion of the Paper in which the Monitors were said to have been found, during a war of unprecedented magnitude, to be both shot-worthy and sea-worthy; and to be confessedly unequalled in their power of penetrating other vessels and of resisting penetration themselves. With all deference to the Author, he joined issue with him as to the efficiency of the Monitors as vessels of war. Mr. Welles, the Secretary of the United States navy, in one of his earliest Reports, dated December 1st, 1862, thus set forth the purposes for which the Monitors were built, and which Captain Hamilton thought at once disposed of the question as to their seaworthiness, as that quality was not claimed for them:—

> "In the construction of iron-clads of the Monitor class, the nautical qualities of the vessel have not been the governing object, for with light draught and heavy armament, high speed is not attainable. But they are adapted to the shallow waters of our coast and harbors, few of which are accessible to vessels of great magnitude. While the larger armored vessels, with their heavy armament, cannot nearly approach our shores, those of the Monitor class can penetrate even the inner waters, rivers, harbors, and bayous of our extended double coast."

It could not be expected that Mr. Welles would tell the whole truth at the time this extract was written. Had he done so, the Confederates would have learned that the Monitors were intended to effect the easy reduction of the defences of the harbour of

Charleston and the occupation of that city; after which the whole sea-board of the Confederate States was to be subdued by these presumed invincible and invulnerable vessels.

The first engagement between the 'Monitor' and the 'Merrimack' was at Hampton Roads. The 'Merrimack,' a 50-gun steam-frigate, had been burned to the water's edge by the Federals when they abandoned Norfolk. The Confederates succeeded in raising this wreck, and, finding it uninjured from the water line to the keel, conceived the idea of converting it into an iron-clad floating battery for the defence of Norfolk. This was accomplished simply by roofing over what was left of the original ship, so as to leave a kind of central casemate. Upon this roofing, which was of stout timbers of oak, was placed two thicknesses of 2-inch plates. The roof was pitched at an angle of about 30°. With this untried and purely experimental construction, Admiral Buchanan made his celebrated raid upon the wooden vessels of the Federals in Hampton Roads, and succeeded in destroying two of them. The 'Merrimack,' in clearing herself from the 'Cumberland,' which vessel she sunk by ramming, wrenched her attached prow, which was no part of the original construction, and caused the ship to leak badly. Nevertheless she encountered boldly the 'Monitor,' a vessel constructed with all the, by no means insignificant, mechanical resources of the United States. But as the presence of the 'Merrimack' in Hampton Roads was to prevent General McCleland making it the base of his operations against Richmond, it was necessary for her to show fight, under any and every circumstance. Hence she fought the 'Monitor.' Both sides claimed the victory. In the Official Report of the Commander of the 'Minnesota,' who witnessed the battle, were these significant words:—"For some time after this, the Rebels concentrated their whole battery upon the tower and pilot-house of the 'Monitor,' and soon after the latter stood down for Fortress Munroe; and we thought it probable she had exhausted her supply of ammunition or sustained some injury." The 'Monitor' thus took refuge in the shoal water, and under the guns of the fortress. The 'Merrimack' returned to Norfolk to repair her prow, and to renew her supply of ammunition, which had been exhausted by the two days' fighting. Admiral Buchanan having been severely wounded by a rifle-bullet from one of the batteries on shore, while standing on the roof of the 'Merrimack,' Commodore Tatnall took the command of the 'Merrimack,' and offered battle to the 'Monitor,' which she never accepted, being careful to keep in shoal water. General McCleland, however, was forced to make York River the base of his operations. This compelled him to pass through the swamps of the Chickahominy, where he was beaten by General Lee in the seven days' battles around Richmond. Unfortunately for the Confederates, the 'Merrimack' drew too much water (22 feet) to pass up the James

River, and to hold it against the Federal gun-boats, which subsequently rescued McCleland's army. When Norfolk was abandoned by the Confederates, the troops being required for the defence of Richmond, there was nothing left but to destroy the 'Merrimack,' which was effectually done. In the action between the 'Monitor' and the 'Merrimack,' neither vessel seemed to have been penetrated by the guns of the other. The secret of the escape of the 'Monitor' was revealed in a letter from Captain Brooke, chief of the Naval Ordnance Office at Richmond, and which was dated March 12th, 1864. He said :—

"I have, in common with many persons, a great dislike to mystery; but we have during this war reaped considerable benefit from moderate reticence, particularly in regard to guns. The Federals, probably judging by themselves, did not suspect that the Confederates would permit the power of their ordnance to be underrated, when the simple statement of the truth would have satisfied the world, that the Monitor's escape in her contest with the Merrimack was simply due to the fact, that the latter, not anticipating an encounter with an iron-clad, was not supplied with solid shot for her rifle guns. It was difficult at that time to furnish even a sufficient number of cast-iron shells. The United States exulted beyond measure in what they supposed to be the undoubted proof of the efficiency of their Monitors; and although with ordinary prudence, in view of possible improvements in ordnance, they added a few inches to the plating of their turrets, they expended precious time and enormous sums of money in building a swarm of Monitors, several of which were ventilated by 7-inch rifle bolts from our cast-iron guns at Charleston. Even the 10-inch shot from Columbiads (a shell-gun properly) battered and bruised their turrets to such an extent as to prevent their working."

In confirmation of his statements, he would refer to the 'Report of the Secretary of the Navy in relation to Armored Vessels,' a volume printed by order of the Congress of the United States in 1864.

The first serious order for the attack of Charleston seemed, by this Report, to have been given by Mr. Gideon Welles, on the 6th of January, 1863. Addressing the late Rear-Admiral DuPont, under that date, he said (p. 52) :—

"The New Ironsides, Passaic, Montauk, Patapsco, and Weehawken (iron-clads), have been ordered to, and are now on the way to, join your command, to enable you to enter the harbor of Charleston, and demand the surrender of all its defences, or suffer the consequences of a refusal."

This very peremptory order was not attempted to be carried into effect until the 7th of April, 1863. On the 8th of that month, Rear-Admiral DuPont (than whom a more accomplished and gallant officer never lived), reported (p. 55) :—

"I yesterday moved up with eight iron-clads and this ship, and attacked Fort Sumter, intending to pass it and commence action on its north-west face, in accordance with my order of battle. The heavy fire we received from it and Fort Moultrie, and the nature of the obstructions, compelled the attack from the outside. It was fierce and obstinate, and the gallantry of the officers and men of the vessels engaged was conspicuous."

The result of this action was given in his detailed Report, dated April 15th, 1863 (p. 58), in which he stated that at 4·30 P.M., the battle having commenced at 2·50:—

> "I made signal to withdraw from action, intending to resume the attack the next morning. During the evening the commanding officers of the iron-clads came on board the flag-ship, and, to my regret, I soon became convinced of the utter impracticability of taking the city of Charleston by the force under my command. No ship had been exposed to the severest fire of the enemy over forty (40) minutes, and yet in that brief period, as the department will perceive by the detailed reports of the commanding officers, five of the iron-clads were wholly or partially disabled; disabled, too, (as the obstructions could not be passed,) in that which was most essential to our success—I mean in their armament, or power of inflicting injury by their guns. . . . I had hoped that the endurance of the iron-clads would have enabled them to have borne any weight of fire to which they might have been exposed; but when I found that so large a portion of them were wholly or one-half disabled by less than an hour's engagement, before attempting to remove (overcome) the obstructions, or testing the power of the torpedoes, I was convinced that persistence in the attack would only result in the loss of the greater portion of the iron-clad fleet, and in leaving many of them inside the harbour to fall into the hands of the enemy."

The nature and amount of the injury sustained by the Monitors was set forth in a Report dated April 24th, 1863, and addressed to Mr. Welles, by the commanding officers of six of these vessels. They stated that, "It had been proved that any heavy blow on the turret was apt to disorder and stop it; that our side-armor and decks were penetrable, and the pilot-house, where is the steering apparatus, and from which is the only lookout, could be made untenable, as two of them to a great extent had been." They also complained that, as the speed of the Monitors was not more than four knots an hour, they lost their steerage-way as soon as the screws were stopped. The inventory of the dilapidations of each vessel (p. 88) fully sustained the statement made by the Admiral commanding the fleet.

Passing to the consideration of the guns employed in this battle on each side, it appeared from the Official Report (p. 75), that there were mounted, on the eight Monitors and the New Ironsides, twenty-three guns of the following calibre: Seven 15-inch smooth bores, charge 35 lbs; feurteen 11-inch smooth bores, charge 15 lbs. and 20 lbs.; and two 150-pounder Parrot rifles, charge 16 lbs. The total number of rounds fired was 139, at ranges varying from 550 yards to 2,100 yards. It also appeared, from the reports of the officers commanding the different vessels, that the number of times the vessels were hit was 256; the least number received by any one vessel being 14, and the greatest, 53. The Official Report addressed to General Beauregard, by that distinguished artillerist General Ripley, who commanded the defences of Charleston, who perfected and created those defences, and under whose anxious supervision the troops were prepared for battle, contains these facts (p. 5) :—

> "In this, the first trial of the Federal iron-clad fleet against brick fortifications, and their first attempt to enter the harbour of Charleston, in which they were beaten before their adversaries thought the action had well commenced, they were opposed by seventy-six pieces in all, including mortars. Thirty-seven of these, exclusive of mortars, were above the calibre of 32-pounders. The expenditure of shot against the fleet was twenty-two hundred and twenty-nine projectiles, of which over sixteen hundred were above the calibre of 32-pounders."

The guns on the Confederate side were thus distributed as to calibre, according to General Ripley's report (p. 11) :—

Ten . .	10-inch	Columbiads,	charge	16	lbs.
Three .	9- ,,	Dahlgren,	,,	10	,,
Two . .	7- ,,	Brooke rifles,	,,	16	,,
Nineteen	8- ,,	Columbiads,	,,	10	,,
Seven .	70-pounder	rifles,	,,	8	,,
Eight .	64- ,,	,,	,,	6	,,
Eighteen	32- ,,	smooth bores,	,,	8	,,
Nine	10-inch	mortars.			

In firing from these guns the twenty-two hundred and twenty-nine projectiles above recorded, 21,093 lbs. of powder were used, or less than an average of 10 lbs. of powder per gun. Of the guns employed on the Confederate side, all were of cast iron, and, except the 7-inch Brooke guns, of an old model, and manufactured previous to the war. The 70-pounder rifles, and also the 64-pounder rifles, were old smooth-bore guns, banded and rifled. The effect produced by this ordnance, which was now obsolete, fired with no charge greater than that used with the 68-pounder smooth bore of the English service, and in the far greater number of guns with charges less than that of the 110-pounder Armstrong gun, a class of ordnance considered obsolete, or, at least, unfit to fulfil the requirements of the service at the present day, had been described.

The Confederates used neither steel shot nor shell. Their projectiles were of cast iron. The battle was won, not so much by the efficiency of the ordnance employed, as by the concentration and rapidity of fire, combined with accuracy, and the excessive sluggishness of their enemy, in movement and fire. General Ripley estimated the distance at which the battle was fought at from 900 yards to 1,900 yards, and that, by laborious preparation, works and material, never originally intended to stand such an attack, enabled the gallant and well-instructed officer and men to gain their end with comparatively small loss.

Admiral DuPont, at his own request, was relieved by Admiral Dahlgren; and the latter, so early as September 8th of the same year, wrote to the Navy Department at Washington thus (p. 239):—

> "Conformably to your directions, I will cause weekly reports to be made of the injuries sustained by the iron-clads. The heavy shot fired which have struck have generally been 10-inch, and are well borne at 1,200 yards; but when the distance is less than 1,000 yards there is a marked difference. The shot which struck the top of the Catskill's pilot-house on a glance, killing Captain Rogers and Paymaster Woodbury, must have been a 10-inch. To prevent similar accidents, the plate should be strengthened and have an interior lining. The shot which struck the turret of the Weehawken at the base came from Moultrie, and was probably a 10-inch. It detached a portion of the interior lining, which broke Captain Badger's leg. The decks always suffer severely, and two or three of the Monitors are now in need of repairs from this cause. The Catskill has been eighteen days at Port Royal under repairs; when she returns another will be sent. Ensign Johnson was slightly hurt last night in the turret by a bolt."

Captain Hamilton need hardly add that, after this, the naval attempt on Charleston was never renewed, not even when Fort Sumter was reduced to ruins by the siege rifle-guns of General Gilmore. No enemy's hand hauled down the flag that had flown over that brave old fortress for four weary years. Its ruins were held until the city of Charleston was evacuated, and the end had come.

On the 17th June, 1863, about two months after the defeat at Charleston, two of the Monitors the 'Weehawken' and the 'Nahant' caught the 'Atlanta,' a Confederate iron-clad, when a-ground in one of the sounds on the coast of Georgia. Unable to move, and at best ill-fitted even to fight a Monitor, after a short action, in which she lost many of her crew from the pitch-pine splinters of her defective backing, she surrendered. This was the solitary naval achievement of the Monitors during the war. To give an idea of the vessel thus vanquished, reference was made to the Report (pp. 205, 206, 207, and 208,) by a board of Federal officers, who minutely inspected the vessel after her capture, described the effect produced by the guns of the 'Weehawken,' and stated their opinion of the armour with which the Confederate iron-clad was plated. Considering that the fire of the guns of the 'Weehawken' was delivered at 300 yards, it did not seem that the penetration, or crushing effect of these 15-inch guns was anything wonderful, and according to Admiral Dahlgren, as quoted by Admiral DuPont (p. 272), the life of these guns was three hundred rounds; some of the guns, however, exceeded that number of rounds. The 'Atlanta' seemed to have been, in spite of her Noah's Ark construction, quite equal to reaching Philadelphia from Port Royal, a distance of 700 miles by sea, and in the performance of which voyage the 'Monitor' went to the bottom. Of the vessels which took part in the siege of Charleston, the 'Weehawken' sunk at her anchors in a moderate gale, how or why there were different opinions. The 'Kentuck' went down the day after the fight of the 7th of April, having been riddled by Fort Sumter. One other Monitor, the 'Patapsco,' it is believed, came to the somewhat inglorious end of having been blown up by a torpedo made of an old beer-barrel.

The conclusions which Captain Hamilton thought might be drawn from these observations were:—First, that the Monitors failed to accomplish the purposes for which they were created, and in their defeat at Charleston, where the action was purely of artillery—forts and batteries against iron-clads—obstructions and torpedoes not having come in play,—the Monitors gave unmistakable evidence of their inability to resist the concentrated and rapid fire of guns with comparatively light charges. Secondly, from the official report of the number of rounds fired by the Federal fleet, as compared with the number of rounds fired by the Confederate forts and batteries, that was 139 to 2,229, rapidity of fire was not one of the qualities of the Monitor guns. And, thirdly, that if the Monitors had been exposed in any of the fights in which they were engaged to the fire of 7-inch and 9-inch Woolwich guns, fired with steel and chilled projectiles, and charges of 25 lbs. and 35 lbs. of powder, their destruction would have been inevitable; and if at Charleston the seventy-six guns had been of the description now finally adopted for the English service, fired with the same accuracy, that not one of the Monitors or iron-clads engaged in that attack would have survived.

Captain Hamilton had proposed to refer, somewhat in detail, to the action at Mobile, between the fleet under the command of Admiral Farragut and the Confederate iron-clad 'Tennessee.' He would, however, simply state that that victory for the Federals was achieved by the wooden corvettes with their nimbleness of movement, and smothering effect of heavy broadsides of shells. Closing around their unwieldly and sluggish adversary, they rapidly poured their broadsides into her, and rammed her until the crew were exhausted by the concussion, and blinded by the smoke of bursting shells. The port shutters were jammed so as to remain so, the funnel shattered, the smoke and gas from the furnaces filling the gun-deck, and finally, after an hour's hammering, the steering gear being shot away, and the Admiral wounded by a shell which entered one of the ports, she surrendered. Not a shot penetrated her armour, which was placed at an angle of 30°, in three thicknesses of 2 inches each, on 30 inches of oak, and against which 9-inch, 11-inch, and 15-inch shot were hurled with charges of 12 lbs., 20 lbs., and 35 lbs., at distances not greater throughout the action than 500 yards, and at some times not more than 10 yards. The loss on board the wooden vessels was great. The Monitors seemed to have occupied the outer line of attack, and were entirely overshadowed by the wooden vessels in this action, one of the latter being the Admiral's flag-ship. If he might be permitted to venture on an opinion upon this vexed question of armour-plated vessels, he thought there was an over anxiety to protect the fighting portion of the crew, at the expense of fresh air and invulnerability of the hulls of the vessels. The plated pilot-house he felt quite certain, with its restricted vision, in the smoke and heat of battle would prove a nuisance. It was a question if a battery of the most efficient guns mounted, *en barbette*, even on an open deck, which was borne by an invulnerable hull, would not, in spite of the possible loss of life which might ensue, do the better fighting in the end than a Monitor with the temperature at 140° on the berth-deck and in the turret. The greatest naval victories in the late American war were gained by wooden vessels: commencing with Hatteras Inlet, including New Orleans, and ending at Wilmington. He considered that if the hulls of war vessels were made invulnerable to shot, that would be sufficient; and by allowing the men to fight in the open air, with plenty of guns, such vessels ought to be more than a match for Monitors.

A question had been asked as to the velocity of the 15-inch American shot. He regretted that he could not give any official data directly bearing on that point, as his experience with that gun had not been under circumstances propitious for minute and scientific observations. He found, however, in the last edition of the U.S. Navy Ordnance Instructions, published by the Navy Department at Washington, the ranges of the 15-inch gun, together with the time of flight of the projectile. Perhaps a comparison of these ranges with those of the Armstrong 300-pounder (he had no such table of the Woolwich guns) might throw some light on the relative efficiency of the two guns. The range of the American gun, with 35 lbs. of powder, at 5° of elevation, was 1,700 yards; the time of flight being $5''\cdot7$. The range of the Armstrong gun, with the same charge and at the same elevation, was 2,230 yards; the time of flight being $6''\cdot5$. Taking the time of flight as the measure of range, the flight of the Armstrong projectile in $6\frac{1}{2}''$ was 2,230 yards, and that of the American gun, in the same time, was 1,900 yards, so that the difference in favour of the Armstrong gun was 330 yards in one case and 530 yards in the other.

Mr. W. CAWTHORNE UNWIN said, he wished to call attention to two points in connection with the penetration of plates:—first with regard to the resistance of plates placed at an angle to the line of fire, and next to the resistance of laminated targets, composed of a series of plates superposed one upon the other. The formula of

Captain Noble giving resistance to penetration of vertical plates was carefully deduced from the experiments, and very nearly expressed the actual results. But the formula for the resistance of inclined plates, referred to by Mr. Bramwell, rested on an entirely different basis. It was, in fact, a speculative formula based on the assumption that the resistance to penetration would vary as the square of the thickness of the plate in the line of fire, or inversely as the square of the sine of the angle of incidence. This formula was only partially confirmed by the few experiments quoted by Captain Noble. A careful examination of these results, and of the experiments with cast-iron shot against inclined plates, made by the Iron Plate Committee, led him to believe that the resistance, with hemispherical-fronted shot, did not increase in nearly so high a ratio, whilst for ogival-ended shot the resistance, except at very low angles, would probably be found to agree still more nearly with that of vertical plates.

It had been stated that the resistance of laminated targets was only in proportion to the sum of the squares of the thicknesses of the separate plates, whilst the resistance of solid plates was as the square of the total thickness. But the experiments at Shoeburyness against the target designed by Mr. Hawkshaw, which was composed of six $\frac{5}{8}$-inch plates, faced with a plate $1\frac{1}{2}$ inch thick; and the experiments in America on targets composed respectively of twelve, six, and four 1-inch plates, exhibited a resistance which, whilst it fell very much short of the resistance of solid plates of equal thickness, was nevertheless considerably greater than the sum of the resistances of the separate plates.

Captain Symonds, R.N., said he should be sorry if the idea went forth that had been introduced in the Paper, that naval officers had a strong objection to the turret system, on account of the bad accommodation it afforded them. He was quite sure those who knew anything at all about naval officers would be satisfied that such an objection did not exist. He believed their principal objection was to the want of accommodation for the men. Now, it was quite certain there was a most anxious desire to obtain the best fighting machine, but it was necessary that the men who had to fight that machine should have a proper and healthy home to live in, or it could hardly be expected that the vessel would be fought properly. He believed the absence of this was the principal objection to the turret system, as hitherto applied, and more especially in the example brought forward by the Author of the Paper. There had been a multitude of objections raised to that vessel on the score of bad accommodation and sea-going qualities. Mr. Scott Russell and several other speakers had nearly exhausted the subject; he would, therefore, confine himself to two or three practical questions, which Mr. Bourne would probably reply to.

In the first place he thought the question of ventilation was quite disposed of. It had been shown that the ventilation of many of the Monitors was so imperfect, that suffocation had nearly ensued. It was dependent upon so many contingencies that one would hardly like to trust to artificial ventilation in a sea-going ship. In the next place, he could not perceive how the anchor-gear of that ship was to be worked properly even in moderate weather, and particularly in the event of a storm. Again, if a man fell overboard, although the boats were very nicely placed on the davits in the drawing, it did not appear how any one was to stand on the deck to lower them, therefore nothing could be done to save the man. He thought, in dealing with this subject, it was desirable to mention these practical points, which were of vital importance, though it might appear rather going into details. As to rigging a vessel of that description for sea-service, he would pronounce the idea, with great deference to the Author, preposterous. In the first place, even in the comparatively smooth water of the Channel, it would be impossible for the men to work the sails in a moderate breeze; therefore that idea must be given up *in toto*. He thought the information given by Captain Hamilton was valuable on many points connected with the Monitors. It had anticipated much that he intended to say, and confirmed a great deal of what he had heard from officers both of the Northern and Southern States of America, who had operated in and against them, not only as to their not answering as sea-going vessels, but also as to their being excessively unpopular with the seamen. Therefore he thought that, so far from their being generally regarded with a favourable eye as sea-going ships, the contrary was the fact. That such vessels might be useful in the broad and deep waters of the American rivers was a point he could not offer a decided opinion upon, because practical men had adopted them for special service there; but for the narrow waters of the English sea-board, and in various well-known rivers, a vessel of such dimensions, draught of water, and character, he considered would be altogether out of place. In the first place, they would not have space to manœuvre in, and in the next, there was no requirement for so large a vessel. He had no doubt a smaller description of vessel of light draught might be made, to carry heavy ordnance, for the purpose of defending harbours and the mouths of rivers. If vessels of light draught of water could be adapted for coast defence, he thought there were many in the Royal Navy which might be altered to comply with the requirements of the case, so as to carry the guns at present existing, and that such altered vessels would be more formidable to any approaching hostile squadron than a vessel of the description under consideration.

Mr. Bourne had asserted, that no material improvement had been made in broadside ships beyond making them rectangular boxes. He thought that remark was rather hard upon those who had designed the broadside ships of the present day; for, if Mr. Bourne had looked more into the matter, he would have found that there had been a great number of improvements beyond applying armour and making the ships rectangular boxes. In the first place, there had been a valuable addition in the shape of 'end-on' firing, a point of the greatest possible importance. The system of combined 'end-on' and broadside firing had been applied to several ships, and he believed it was to be carried out to a still greater extent. The value of 'end-on' firing was in the early stage of the turret history much relied upon. Captain Coles had stated it to be his strongest point; yet it would be found from the official reports, that in the case of the best cupola ship he knew of, the 'Royal Sovereign,' in which all possible obstructions were removed from the deck, the guns in the foremost turret could not fire within 12° of the line of the keel. That was a considerable divergence for a ship not being a sea-going ship, but specially fitted for that purpose; whereas in broadside ships fitted for the purpose, 'end-on' firing was obtained in the way partially adopted in the 'Pallas' and other ships. He referred to the diagram of a ship with the same hull as the 'Royal Sovereign,' with what the Americans called a block house on deck, having angulated ends, in which there were three guns bearing absolutely 'end-on' at both bow and stern. He considered regard should be had to the feasibility of making such broadside ships quite as effective on all points, if not more so, than the turret with the present class of guns. In calculating the weight of the turret-ship's broadside, he had invariably seen the figure 600-pounder used. He suggested that that was not a fair comparison, as there were no 600-pounders which could be relied upon at the present moment; not that he doubted but that they would be eventually constructed. The largest guns, he believed, were 300-pounders, and those, he thought, were the guns to be dealt with for present purposes. Therefore, in calculating the broadside of a ship, whether turret or otherwise, these 300-pounder guns should be used, and in that case the weight of the turret broadside would be materially diminished, the broadside ship having two sets of guns, or two broadsides, instead of only one. Admiral Elliot, and other naval men said the same, argued that line-of-battle ships would in future be most likely employed as they had been hitherto, viz., as a fleet, and would be placed in a position where both sides must be fought. Take the case of a turret-ship in such a position with five guns, three on one side and two on the other, and then take the broadside ship with five guns on either side, the latter would be doubly effective.

To return to the question of the improvements effected in broadside ships of the present day:—There were several vessels now fitting in the Royal Navy capable of firing three, if not four guns in the line of the keel. That was a point which he believed no sea-going turret-ship of the present day had yet attained. From what he could learn, as to the proposed position of the two turrets in the 'Captain,' each of which was intended to contain two 600-pounders—when they were made, the two guns in the bow turret would train at 23°; there being a forecastle between the turret and the stem of 90 feet, obstructing the fire through that arc. In the same way, at the stern, there was a poop which obstructed the line of fire to an angle of 25°. When that ship was going up a river, or entering a roadstead, with an enemy right ahead of her, she would have to yaw, or diverge from her course, so that she would expose fully a hundred feet of her side as a target to the enemy, before she could fire a single gun from the turret; whereas, a broadside ship, of the type he proposed, would be able to keep her course, and fire four protected guns right ahead, viz., two from the main-deck, and two from the forecastle, with a 100-pounder main-deck gun, unprotected, as a chase gun. It would be remembered that, when the turret system was first advocated, it was insisted that 'end-on' firing could be obtained in that system alone, without the divergence of even half a point from the straight course; whereas in the 'Captain,' the first sea-going turret-ship, it was found impossible to fire within 23° one way, and 25° the other: the same might be said of other so-called sea-going turret-ships. Therefore before naval officers were accused of opposing the turret system for any one particular reason, it would be well to consider the points he had mentioned, especially as applied to sea-going ships; and he thought it must be admitted, that there was some reason for not altogether receiving the doctrines which had been put forward by the Author, more especially with regard to Monitors as sea-going ships. He considered Mr. Bourne had failed in laying down as a rule that these vessels possessed sea-going qualities, and in showing that the British broadside fleet required such an expensive and very uncertain set of consorts as the Monitors must prove in a seaway.

Admiral Sir E. BELCHER said, not having heard the Paper read, he could not enter fully into the matters connected with it; but he most entirely disagreed with Admiral Elliot's scheme of fighting ships. It had always been the system in the Royal Navy to look the enemy in the face, and not fight him over the stern. It was a maxim of Nelson's that "when an officer found himself at fault, he could not err if he put himself alongside of his enemy." Now, with regard to the classification of the Royal Navy, Admiral Elliot had placed the line-of-battle ship's speed, the very essential point for chasing, as No. 5 on the list of qualities. How was it possible to capture the enemy, being a ship of the line, if he could not be come up with when chased? To go back to the time of the former war, at the commencement of this century, there was the case of the 'Guillaume Tell.' She was a fourteen-knot line-of-battle ship mounting 104 guns, though she was called an 80-gun ship. The 'Lion,' an unfortunate English ship of 50 guns, went alongside of her, but was soon dismantled and dropped astern. The 'Penelope,' a smart sailing frigate commanded by the gallant Sir Henry Blackwood, kept her position, first on one quarter, then on another, delivering her broadsides at each tack, and keeping up such a well-directed fire, that she cut away her spars, and eventually left nothing above but the mainyard hanging to the mainmast, shattered fore-topsail and fore-sail; then a ship taken from the French, the 'Foudroyant,' came up and settled the business. Had there been no frigates and ships of the line competent to chase and engage vessels of equal size, the 'Guillaume Tell' would never have been captured. In another instance, the 'General d'Hautpoult' (later the 'Abercrombie') was chased by the whole of the West India squadron, and would have escaped by her superior sailing, but the Pompée (also a French ship) kept close up with her, and the 'Castor' frigate, and 'Recruit,' 18-gun brig, the latter commanded by the late Sir C. Napier, injured her so much by cutting away her light sails, that the 'Pompée,' running alongside, took her in twenty minutes. The 'Abercrombie,' in company with 'Belle Poule,' 'Dryad' and 'Armide,' chased the famed American runner, the 'General Gates.' The 'Belle Poule,' also of French construction, was the fastest frigate in the Channel fleet, and with her consorts had chased the American for three days, from Bordeaux till they came close to L'Orient; and she would have escaped, but for this 'Abercrombie,' which dropped down under topsails and top-gallant sails, and took her with the greatest ease. Therefore it was necessary that high speed should be a first quality of a ship of the line more than of any other class of vessel. It was so in the French navy. In the war from 1812 to 1816, the 'Malta,' 'Abercrombie,' 'Superb,' 'Pembroke,' 'Warspite,' and others were as fast as any frigates, and he did not see why frigates should now be substituted, which, on an average, did not exceed a speed of 12 knots an hour. He was satisfied hereafter ships of the line would beat all other vessels for speed. The 'Warrior,' of 7,000 tons, was the fastest ship in the navy; and if some of the armour of the 'Warrior' were taken away, and four of the 600-pounders, which were promised at some future day, were mounted on her, she would then be a vessel fit to take the line, although not a line-of-battle ship.

With respect to the Monitors, he would say that he had been accustomed to try the effects of ramming floes of ice in the Arctic seas, and had succeeded in rupturing the ice, the weight of the vessel under his command pressing it asunder. If he had to deal with a Monitor, he would calculate its weight, as compared with that of his own vessel; and if the displacement one-third forward was one-fifth of the weight of his vessel, he would walk over her. He could not fancy that the Monitors, so nearly even with the water's edge, would ever be adopted in the English navy. He was satisfied none of the rams would ever effect the objects for which they were intended. He thought, instead of going into the enemy, the ploughshare noses would go below, from the extreme end of the vessel being so overladen, and not being sufficiently buoyant, and would in most cases sacrifice themselves.

With regard to ships of the line, he did not see any vessel, as yet, in the naval service, established as a line-of-battle ship. Therefore it might be said vessels of 4,000, or 5,000 tons, would be termed ships of the line. Formerly, in naval warfare, 50- or 60-gun ships were not allowed to take the line, as a rule; but on one or two occasions they had done so. He supposed now, that the number of men carried, the rank of the captain, and the tonnage of the ship, would constitute the liner. He hoped the principle enunciated by Admiral Elliot, to fight stern on, so as to be in position to run away from the enemy, would not be considered to be the feeling of the British navy.

Mr. John Macintosh said he had listened attentively and with considerable interest to what had fallen during this discussion, and more particularly to the opinions expressed by naval officers with reference to the Monitors and turret ships. In the first place, it had been said that these vessels were not sea-worthy; and secondly, that the crews could not live in them for want of proper ventilation. He dissented altogether from these opinions. He considered that the small extent of surface, exposed by the Monitor ships above water, was an important element in their construction, and gave them a great advantage as regarded speed, over vessels that floated with their sides considerably above the water line, and which were further encumbered with complicated top works in the form of masts and rigging.

With respect to ventilation, he maintained that the Monitor and turret vessels were the best ventilated ships afloat, as the draught inwards formed a continuous stream of air, and which was necessarily distributed over the whole body of the vessel. It had been stated that the sea would raise a Monitor vessel, and so expose her salient points to the ram of an opposing vessel; but he considered it would be impossible for a swan-breasted ram to injure a Monitor. He would ask what was the effect produced by cutting vessels down to the water's edge but to reduce their buoyancy, and so prevent their rising with the wave? On the contrary, a ship whose sides were elevated above the water possessed great buoyancy, and readily lifted with the waves, and was what sailors termed 'lively.' No doubt, as had been observed, officers, as well as the men, liked to have roomy ships and comfortable berths, but, in his opinion, this desire was too often gratified at the cost of efficiency and speed. Such vessels as had been advocated as the proper type for ships of the line, he contended would have no chance in battle with vessels of the Monitor class. Broadside guns required large port-holes, which afforded easy access to incendiary and other projectiles, and the guns were, moreover, very circumscribed with respect to their direction of fire. In Monitor vessels, on the contrary, the guns revolved with the turret, and could fire in all directions, and the aperture required was small. Two of these vessels would, in his opinion, be able completely to annihilate a whole squadron of line-of-battle ships. As to the assertion that the Monitors had no speed, that was a statement to him wholly incomprehensible. If any other practical Engineers received instructions to build a vessel of the greatest speed, he apprehended they would dispense with the upper decks and spars, so as to reduce the resistance opposed to the vessel as much as possible. The Monitors had decks that were as water-tight as the bottoms. They were essentially sea decks; hence no sea could get into the interior. He had an iron yacht that was only 9 inches above the water, 75 feet long, and 6½ feet beam, coming off to a taper aft, and he had ridden out severe gales in her, during which her behaviour was admirable. Instead of rising and tumbling with the waves, the sea swept over without injury. Any Engineer knew if a wave struck a buoyant vessel, the blow of the wave lifted it. Not so, as he had before observed, with these Monitor vessels. They might be exposed to a heavy sea for any length of time, but they would not rise. He did not say it would be advisable to adopt these vessels for the navy generally, but he would have them as auxiliaries; for he believed, for the reasons given, that as regarded ventilation, safety, and speed, nothing afloat could touch them. These vessels were impervious to water, impervious to fire, and, he believed, impervious to shot, and they would be found most potent instruments of destruction in case of war. War had now become a race of invention, and the nation most distinguished for fertility of resources, and the application of new materials and expedients, would become the most powerful both in attack and defence. Another advantage attending Monitor ships was, that they required no men to reef topsails or to work on deck; and they, moreover, had nothing on deck which, by being injured, would impair their efficiency, whereas a few shells would render an ordinary vessel totally helpless. He considered if England was to maintain her supremacy afloat, she must follow the inventions of the day, and not adhere to an old form of construction no longer suited to the emergencies of the times.

Mr. G. W. Hemans did not wish to interrupt this technical discussion further than to say that, although as Engineers they might not fully comprehend all the nautical points brought forward with regard to the sailing qualities of different ships, yet he was sure it would be a subject of interest to all present to be informed in what way these low-decked ships were ventilated.

Mr. J. M. Heppel said, although the subject of ventilation had been more than once incidentally mentioned in the course of the discussion, he did not think the point to which he would call Mr. Bourne's attention had been specially referred to—that was, as to the effects that would be produced by the introduction, on the part of an enemy, of a vitiated atmosphere within those ships. Whilst he listened to the recital of the exploits of the Monitors during the American war, it struck him that they were peculiarly open to an attack of that kind from the enemy; and, drawing all their ventilation by artificial means through certain small orifices, if, in the course of the march of invention, any kind of shell should be produced which, from being exploded at the proper time, would diffuse vitiated gases in the vicinity of those orifices, the ventilation might be completely destroyed. He had casually consulted Dr. Letheby and Sir W. Armstrong on this point. The former eminent chemist unhesitatingly gave it as his opinion, that it would be an exceedingly effective method of attack, and that if gases of that kind were discharged in large quantities near the ventilating orifices, it would render the interior of the ships untenable, and in that way it would be comparatively easy to poison a whole fleet. Sir W. Armstrong was not quite so positive in his opinion. He thought, to render it perfectly effective, the shell itself would have to be projected into the interior of the vessel, and that there might be difficulty in throwing the shells with sufficient accuracy to produce the desired effects. He alluded to this because the subject of ventilation seemed to him to call for very complete investigation. He had no doubt every point of this kind had suggested itself to the Author, who, he hoped, would state his views on this part of the subject. It struck him that before proceeding to organise the construction of this kind of vessel on a large scale for this country, it was important to consider whether they were not only liable to bad ventilation during their voyages, but also to hostile operations of the kind he had suggested on the part of an enemy in action.

Mr. E. A. Cowper had lately received information from one of the managers of the Horten dockyard, in Norway, where Monitors very much like those described by the Author were being constructed by the Swedish Government. The water-line in those vessels was only 16 inches below the deck, and the turret was of almost exactly the same construction as the American turrets, being, in fact, made from Ericsson's drawings. One most important detail of construction in the Swedish Monitors was, however, not shown in the drawing of the 'Dictator' (Plate 8). This was a large, square, wrought-iron box, formed immediately under the turret, to support its weight when resting on the deck, the lower part being worked in with very strong sections, or girders, running for and aft, whilst in the centre, and immediately under the step for the toe of the shaft, the keel was made remarkably strong. The central vertical shaft was 13 inches in diameter, and had diagonal ties from its upper end down to the base. The turret itself rested upon turned brass fittings on the deck, there being a groove, or gutter in the brass ring, with drain pipes to it, to take away any water that might come in through the small crack or opening when the turret was lifted slightly. When it was required to turn the turret, a large key was driven in under the step, by means of a 'tup,' or hanging weight, and thus the turret was lifted perhaps about a quarter of an inch clear of the brass ring, so that it could turn freely. The turning was effected by means of donkey-engines, with link motion and without any fly-wheel, the reversing lever of the link motion being worked as desired by a man inside the turret; the gun was thereby pointed as required, and then, as soon as it was fired, it was turned away from the enemy, for the purpose of loading.

Special means of supplying the crew and the boilers with air were provided, as ventilation was of the utmost importance in such vessels—he could not call them ships; for, without some artificial means of constantly throwing in a large quantity of air, the men could not live, nor the boilers make steam. Two large fans were placed in the square water-tight box before referred to, the fans being driven by donkey-engines provided for the purpose. The first of these Monitors had been at sea for a short time, and it was found that sufficient air was admitted as long as the fan-engines were kept going.

The engines for driving these vessels were very compact, though involving the use of two bell-cranks, or rocking-shafts. The cylinders were placed horizontally, and bottom to bottom, the pistons having trunks of small diameter, to allow of connecting-rods taking hold of levers on the rocking-shafts, vibrating on each side of a vertical line; whilst the only crank on the propeller shaft was driven by connecting-rods from other levers on the rocking-shafts.

The framing was, of course, very plain, taking the propeller shaft below, and the rocking-shafts and cylinders above.

The grating at the top of the turret struck him as being very open; and he had listened with great interest to the remarks of Mr. Longridge, as to the possibility of throwing stink-pots into these vessels, as he feared such projectiles (if they could be thrown in) would be most fatal. The grating, in the case of the Swedish vessels, was formed of bridge rails, as a cheap form of iron and readily obtainable. There were two rows of these rails, placed base to base, but with an interval of 1½ inch between them, to allow of ventilation; so that if any shell fell upon them, it probably would not enter. If, however, any suffocating, poisonous, or inflammable liquid were thrown in from the top, it would go into the turret and prove most fatal, as there were no port-holes or openings to which the crew could run for fresh air. It might, perhaps, be arranged that the fans should draw air from some other ventilation funnel or pipe, and blow the stench out through the turret, as well as the smoke of the guns, which would most certainly enter the turret to a large extent when firing to windward; he was not aware whether any such arrangement had been tried. Of course any single ventilation funnel or pipe was, to some extent, liable to be poisoned by the enemy.

Mr. J. Bourne, in reply upon the discussion said, he thought the Institution and the country were much indebted to those who had spoken for the purpose of exposing every possible fault of the Monitor system. If that system was an unsound one, it ought to be the subject of hostile criticism; whereas, if it emerged unscathed from that ordeal, it would have earned a new title to the advocacy of any man who wished that the navy of this country should rest upon a solid basis. As he was not the inventor of the Monitor system, and had never seen the Author of it, it was not to be supposed that he would willingly excuse or extenuate any fault he discerned in it; nor did he imagine that any Engineer would stand forward to assert its propriety, with the confidence with which he now did, on any warranty short of that absolute conviction of accuracy which sprang from full and deliberate investigation.

The objections and the difficulties which had suggested themselves to others had, at the outset, suggested themselves to him; but they had disappeared on a close examination.

The manner in which he became impressed with the Monitor system was this:—In the preparation of a work he was engaged on, he had to make inquiries respecting that system, which revealed to him its great power and importance; and he felt it his duty to disclose the results of those discoveries, seeing it was of such vital importance to the country at large. All great inventions were slighted at the outset. All great improvements were first condemned, next endured, and finally embraced; and that would be the history in this case too. He was quite satisfied that before long the Monitor system, now so discredited, would be adopted not only by England, but by all other nations in the world. When Ericsson's first screw-boat was started in 1837, few people believed in it; and he was ashamed to say he was himself among the sceptics; but he had no desire now to repeat this fault, and he thought it inexcusable that Engineers of ripened experience should allow themselves to be carried away by the force of unreasonable prejudice.

He was anxious that this discussion should have come on when the 'Miantonomah' was in this country, when the theoretical objections of naval men on the one side might have been answered by the practical experience of naval men on the other; but that could not be carried out; and the duty now devolved upon him to reply to the various arguments against the system which had here been stated. He was so assured of the soundness of the views he advocated that, notwithstanding the disparity of debating power on the two sides of the question, and, though he stood alone, he could have no doubt of the result of the discussion, if he could give adequate expression to the facts and arguments he had to introduce.

He had no pet project of his own to recommend, nor did he wish to deal with the question in a narrow and sectarian spirit. It stood before him simply as an interesting engineering problem well worthy of the attention of this Institution, on account of its own national importance, and which he had sought to illustrate as best he could. Whether his views were adopted or not, he was sure the meeting would give him a patient hearing, and would weigh attentively and dispassionately the arguments he brought forward. Thus much he would say—after having listened to all the objections that had been brought against the Monitor system, there was not one which had not previously presented itself to his mind and been dismissed as untenable, and the conviction had forced itself upon him, that either there was but little in those objections, or that his penetration was not sufficient to discover it.

In the first place, he would express his entire dissent from the doctrine that thick armour was a mistake, and that it was better to allow the shot to go through the armour, as it did through the wooden ships in the time of Nelson; and that 5-inch or 6-inch plating to intercept shell was sufficient. He thought every one must see, if the sole function of armour was to intercept shell only, it was hardly worth while to employ armour for the sake of such inadequate protection. Moreover, as was observed by one speaker, if the side were pierced with shot, shells might gain an entrance through the great hole thus made; besides, it would be a most dangerous thing to conclude that in respect of shot and shell the limit of power was now reached, and that no margin should be allowed for that gradual increase in penetrating capability so steadily going on. But this was not all. It was well known the Palliser shell had been projected through 8 inches of armour and 18 inches of backing, or through considerably greater thickness than it had been asserted would keep all possible shells out. Then, could any one fail to see that shot striking a plate at great obliquity would not strike so hard as if it struck it at a right angle; or was it not obvious that the 2 inches of iron on the deck of the Monitors, which was subject only to an oblique fire, would be useless if employed to protect an equal area on the sides of the vessel? Therefore the project of a thinly-clad flotilla broke down with the destruction of the theory on which it rested. At all events, the theory was opposed to facts, and that ought to be sufficient reason for its repudiation.

He was not much alarmed by theoretical terrors of torpedoes, nor could he discern the least difficulty in carrying masts and sails in a Monitor more than in any other vessel, if the displacement were so adjusted as to support their moderate weight. He conceived it was an entire mistake to say, that it had been owing to the introduction of masts and sails into a few turret-ships built in this country, that it had become necessary to make their armour so thin; such thinness being, in fact, mainly due to the considerable height of side, which the want of any special provision to prevent the entrance of water through the deck-openings had necessarily involved, and also to the employment of several turrets, whereby the weight had been needlessly increased. He said nothing as to the expediency of fitting masts or sails in Monitors, or as to how they should be fitted if they were in any case applied, nor that it might not be advisable to fit sea-going Monitors with steam propulsion alone; though, even if this were done, why it should be inevitable that they should consume 1,200 tons of coal in six weeks, or that it would be indispensable that they should carry that quantity, he did not understand. He should have supposed that a Monitor, like any other steam-vessel, would necessarily burn coal in the proportion of the power she exterted and the speed she maintained. If, therefore, she had to make a voyage at full speed to any given point, her daily consumption would be large, but the voyage would be soon concluded; whereas, if her function were merely to wait about watching a harbour, since only steerage-way would have, in such case, to be maintained, the consumption of fuel would be quite inconsiderable, and might be obviated altogether if a sail were hoisted on the chimney, as was habitually done in the early steamboats, and which would be specially easy if the furnaces were out of use. The 'Dictator' carried only 800 tons of coal; but the 'Puritan,' which was a larger ship, carried more, and came more nearly up to Mr. Barnaby's prescribed standard of sea-going efficiency in this respect. With regard to the risks from submarine guns and torpedoes, it was sufficient to say that he had claimed no greater immunity from such risks for Monitors beyond those enjoyed by other vessels, and it was no answer to his argument to assert that, although in all other respects Monitors were immeasurably superior in power to broadside vessels, yet that on one certain point they only stood on an equality. The objection which had been urged in the course of the discussion, that the view from the pilot-house of Monitors would be obscured by the smoke of the guns, would be seen to be groundless, when it was remembered that a powerful indraught through the top of the turret towards the furnaces was constantly maintained, which immediately cleared any lingering smoke away from that spot. This smoke, it was plain, might be conducted, not into the body of the ship, but into the furnaces direct, by means of a pipe provided for this purpose; and any mephitic gases or poisonous liquids which it was said might be projected in shells that would burst on the top of the turret, and stifle the people within the ship, might be conducted away through the same pipe without producing any deleterious effect. The pure air required for respiration by the persons within the vessel was taken into the interior through a tall armour-clad funnel, or pipe, surmounted by a cap which prevents water from entering, and which would also hinder the entrance of a shell or any other foreign body. It was needless to describe the details of the arrangement, whereby the smoke or the fumes could be thus carried away without incommoding even the men within the turret, since any competent mechanic would at once see how this might be accomplished.

It was objected to the lowness of the 'Dictator's' sides, that she would necessarily sink if the bottom were pierced by so large a hole as to overpower the pumps, since the superfluous flotation would not buoy up a filled compartment if the water-tight bulkheads were more than 22 feet apart, which was an amount of closeness impossible in practice. This was, no doubt, a valid objection. But even if it were insuperable, a Monitor would not be worse in this respect than a wooden ship which was without bulkheads at all, and which must necessarily sink if the leak were so great as to overpower the pumps. It would be seen, however, by reference to the section (Plate 8) of the 'Dictator' that if the 'tween-decks were tight, not much water could enter the holds, even if part of the bottom were out of the ship, as the holds would, in fact, be like inverted bottles, where the air hindered the water from entering. He agreed in thinking, that it would never do to cut up the space occupied by the crew with numerous bulkheads; but the crew were all lodged above the 'tween-deck, and there was no objection in dividing the holds, which were only devoted to carrying shot and stores and other things of that kind, by frequent bulkheads, both longitudinal and transverse; and by restricting these bulkheads to the holds, they would be but small, and would not require to be half so numerous as Mr. Reed imagined. Of course Monitors, like other vessels, might be made with a double bottom, if judged advisable, or with snag chambers along the sides to prevent leakage from ramming; and if in America this had not been done, it was simply because it had been found that it was not required to meet any risks there existing.

It had been urged, that the diagram which he had prepared, showing the ram bow of the 'Bellerophon' directly against the side of the 'Dictator,' proved, if the 'Dictator' was only supposed to be sufficiently listed by the collision, that the point of the ram would in reality encounter the naked side of the 'Dictator' below the armour shelf. In this view he also concurred; but it would be very difficult suddenly to list over such a large mass as the 'Dictator,' especially as she had an armour shelf, 4 feet broad, all round her, which, by its inertia, would resist sudden immersion and emersion; the more probable result would be, that the bow of the 'Bellerophon,' as being the weaker object, would be stove in or crumpled up. Even if there should be any doubt of this, however, in the case of the 'Dictator,' there could be no doubt of it in the case of a Monitor with the configuration shown in Plate 8, Fig. 4, which represented a portion of the original 'Monitor' which was assailed by the ram of the 'Merrimack,' without any other result than to disable the assailing vessel. These diagrams were intended, however, not so much to show the futility of ramming Monitors, some of which might be as vulnerable in this respect as other iron-clads, as the futility of the design so confidently vaunted in some quarters, of the practicability of running over them with existing ram-vessels. That a Monitor might not be run over by a species of vessel specially designed for the purpose, he by no means maintained. On the contrary, this was a weakness of the system which he always asserted. Nevertheless, he had thought it would be most difficult to carry out such a method of attack if the Monitor sought to frustrate it, as of course she would do, and it would be quite unsafe to trust to a resource which might turn out to be unavailable in practice.

But objection had been taken not merely to the deficient height of the 'Dictator's' armour above the water, but to its deficient depth below, which was only 4 feet 8 inches, whereas in the 'Bellerophon' and 'Hercules' it was 6 feet, and in the 'Monarch' he thought, still more. Now a deficient depth of armour below the water-line in an armour-clad ship would be detrimental and, perhaps, fatal, under three different conditions; 1st, when the vessel, in rolling heavily, exposed the unarmoured side above water, and shot and shell could be poured through it; 2nd, when the vessel was buoyed on the crests of waves rising high upon her sides, leaving corresponding deep depressions which might reach below the armour, and expose the unarmoured surface to hostile fire; and, 3rd, when a shot was directed obliquely downward through the water to pierce the unarmoured side. Now it was well known that Monitors scarcely rolled at all, while tall iron-clads rolled very heavily, and might probably expose themselves to serious injury by rolling the unarmoured surface out of the water, even though the armour were carried to greater depths. Then it was plain that Monitors could not be buoyed up high out of the depressions by the crests of waves rising against their tall sides, since they had no tall sides; and the water, which in the case of common vessels produced this action, was, in their case, spilt over the deck. Finally, it would be seen by a reference to the cross section of the 'Dictator,' (Plate 8) that the best-directed shot which could be fired through the water against the unarmoured side would, owing to the projection of the armour shelf, fall against the side not 4 feet 8 inches below the surface, but the double of this, or 9 feet 4 inches below the surface. In other words, it appeared that the side of the 'Dictator' was protected against oblique shot by its armour to a depth of about 9 feet 4 inches below the surface, which was a greater depth of protection than was afforded in the 'Monarch,' or any vessel in the British navy.

Mr. Reed said that he imagined Mr. Bourne must have fallen into inadvertence when he stated that the guns of the 'Dictator' would be able to pierce the 'Bellerophon,' while the guns of the 'Bellerophon' would be unable to pierce the 'Dictator,' and that he apprehended it would be necessary to modify that statement. Should this impression prove to be correct, he could, of course, abandon the position at once; and he was willing to leave it to Mr. Reed, and to others who knew more of guns than himself, to say how far his view could be sustained. By a reference to American experiments, recorded at page 141 of Holley's book on Ordnance, it appeared that a target composed of a solid 6-inch plate, backed by 30 inches of oak, was completely smashed and penetrated by a cast-iron shot weighing 400 lbs., propelled from a 15-inch gun by 60 lbs. of powder, at an initial velocity of 1,480 feet per second. The armour of the 'Bellerophon' was 6 inches thick, with 10 inches of teak backing, but there was the skin of the ship besides, which might bring the resisting powers of the two targets to an equality. There did not appear, therefore, to be any very great inaccuracy in assuming that the 15-inch guns of the 'Dictator' would be able to pierce the sides of the 'Bellerophon,' even without the agency of the Palliser projectiles, by which the penetrating power in the case of solid plates was increased. There was no case, however, so far as he was aware, in which, even with the aid of these projectiles, armour had been pierced of a thickness equal to the bore of the gun; and hence if the 10½-inch armour of the 'Dictator' was supposed to be solid, it could not be pierced with the 10½-inch guns of the 'Bellerophon.' No doubt laminated armour was much more penetrable than solid, and part of the 'Dictator's' armour consisted of this, being formed of six layers, of 1-inch plates laid on solid slabs 4½ inches thick. Mr. Reed said that this armour was used, because, at the time, the Americans could not make any other; and in contrasting the essential merits of the two kinds of vessel, it would, perhaps, be right to exclude accidental differences in the quality of the armour arising from defects of manufacture, and to suppose the kind of armour to be in both cases the same. Even if this mode of comparison, however, were disallowed, and the comparison were made with the laminated armour now existing, he had to remark that, while laminated armour was confessedly much weaker than solid against common shot, yet it by no means followed, nor did he believe it would be found to be the fact, that pointed Palliser bolts would exhibit the same superior penetrating power in the case of laminated armour as was shown in the case of solid. In the case of solid armour acted upon by a common punch, the piece punched out was the frustum of a cone, and as the ruptured area rapidly increased with the thickness, the resistance increased in a like proportion; whereas in the case of laminated armour, discs were punched out, making not a conical, but a cylindrical hole, and the resistance was merely as the cylindrical surface cut. In the one case the hole at the back of the plate would be much larger than the punch, and in the other case it would not; and if more extended experience should show that this last result obtained also in cases where solid armour was pierced by pointed Palliser shot, the conviction would be inevitable, that the diminished resistance which in one case was produced by lamination, which arrested conical enlargement, was in the other case produced by the surface of rupture beginning at a point instead of at a circumference, whereby the ruptured area was reduced, and the maximum diameter of the hole was only made equal to that of the projectile. The effect, therefore, of diminished resistance, or increased penetration, might be produced in one of two ways—by laminating the armour, or by pointing the projectile. But it could not be obtained twice over, or, in other words, if the surface cut was already a minimum, as was the case in laminated armour, increased penetration could not on this theory be obtained by pointed projectiles, as the ruptured area could not be of less diameter than that of the projectile. He thought, therefore, there was some ground for the conclusion, that laminated armour was nearly as strong against common projectiles as solid armour was against Palliser shot; and also that, in the case of laminated armour, Palliser projectiles would give no material advantage. And as, so far as he was aware, no Palliser shot had yet succeeded in penetrating a plate equal in thickness to the bore of the gun, he thought there would be no very gross error in concluding, that the 'Bellerophon's' $10\frac{1}{2}$-inch guns would be unable to pierce the sides of the 'Dictator,' with any kind of projectile at present known, stiffened as the armour was by horizontal plates on edge at top and bottom, and by intermediate vertical plate iron brackets, pitched very close both within the ship and without. Apart, however, from these considerations, he did not believe that any one looking at the section of the 'Dictator' could come to any other conclusion than the one at which he had arrived. There was only a height of 16 inches above the water altogether, of which 2 inches consisted of iron plate extending across the deck to a breadth of 50 feet, and 9 inches consisted of teak plank of the same breadth, or in all 11 inches of a target of wood and iron 50 feet thick. No ball could pierce that; and the most trifling ripple on the surface of the water would shield the remaining 5 inches of depth from shot of any kind. That surface, however, was fortified by thick beams of oak, pitched very close together, also extending right across the ship; and he left them to judge what chance there would be of piercing such a target as that with any ordinary gun, even if it could be struck, which would be a difficult matter. The turret of the 'Dictator,' 15 inches thick, exhibited similar strength to the sides, having been carefully proportioned thereto. It should not be understood, however, that he defended laminated armour. What he defended was very thick and strong armour, and very strong and heavy guns, such as could be carried on the Monitor system, and on it alone; and even if the 'Dictator' should be held to be penetrable with the best armour that could be applied to her, that fact would only be an additional argument in favour of the great thickness of armour he recommended.

The information he had received from America was quite confirmatory of Mr. Colburn's statement, that the dynamic value of 1 lb. of gunpowder burnt in the American guns was greater than that of 1 lb. of powder burnt in the English. He did not know that more of the powder was burnt in one gun than in the other, but the efficacy of what was burnt was greatest in the American guns. It had been doubted that two successive balls fired from the $13\frac{1}{4}$-inch gun with 90 lbs. of powder, pierced the 'Hercules'' target of 9 inches thick with backing, and, as other things being the same, the penetration varied as the *vis viva* whether the velocity was great or small; and, as the *vis viva* varied as the powder actually burnt, it might be imagined what would be the effect that would be produced by a Palliser shot fired from one of the 'Puritan's' 20-inch guns with

120 lbs. of powder upon the strongest iron-clad in the British Navy. The racking effect produced by the impact of very heavy shot at small velocities, in breaking the fastenings and stripping off the plates by the rebound of the backing to which Mr. Mallet referred, was quite well understood in America; and one reason for using laminated armour, all riveted together, in a solid mass, was to prevent this action from taking place. Whether laminated armour should be used in combination with solid to prevent the plates from jumping off was still doubtful. But if it should, another argument was added in favour of the power of carrying great collective thickness. He did not agree with Mr. Longridge in thinking, that the power of guns might be so increased that no armour could be got to withstand them. But he agreed with him in believing, that there was no visible limit to the power of guns. He was equally unable, however, to discern any limit to the resisting power of armour, supposing the Monitor system to be adopted. He was glad that Mr. Longridge again proposed to bring the subject of heavy guns before the Institution, and he trusted that the conclusions then arrived at would be pressed with all the authority of the Institution upon that department of the Government which the question mainly concerned. Improvements which addressed themselves to commercial wants might be left to force their own way to popular appreciation and adoption. But improvements which addressed only some want of the Government stood on a totally different footing, and, however valuable, were almost sure to be choked by official cobwebs to the great loss of the community, unless some extraneous force was brought to bear that commanded respectful attention.

He did not think it necessary further to discuss the risks of the threatened Chinese mode of attack by stink-pots, or by the discharge of poisonous liquids, since this, in the first place, was not a danger which would attach specially to Monitors, but would apply to all vessels having openings leading into the interior, through which offensive substances might be discharged; and he had already shown that Monitors presented special facilities for obviating such risks, owing to the manner in which the air required for respiration was introduced. Nor could he discuss the dangers of the ventilating-engine breaking down, seeing that there were several such engines provided which would not be likely to break down together; and seeing further, that even if they did, the chimney alone of a 500-H.P. engine would afford ventilation for a crew of six hundred and twenty-four persons at the very large allowance of 50 cubic feet per head per minute—this quantity of air being requisite to accomplish the combustion of the fuel. Nor had he ever maintained that the navy of England should consist exclusively of floating gun-carriages, or that Monitors were fitted for carrying troops. What he had maintained, and did maintain, was, that in any encounter between a

Monitor and a broadside ship of the same size, the Monitor would necessarily prevail, and that unless the thinly-armoured navy of England were guarded by an adequate number of these steam armadilloes, it would stand in imminent jeopardy in the first naval war of being all sent to the bottom. Admiral Elliot's Table of qualifications proper for ships of war showed that their first quality and their last, their Alpha and Omega, should be the power of fighting; and in that sentiment he quite concurred, and he hoped that it might be universally accepted. He also agreed with Admiral Elliot in thinking that the jet method of propulsion deserved a fair trial. He thought the country owed him a debt of gratitude for his efforts in getting the principle tested and recognized. He hoped to see other naval officers following this example, and allying themselves with intelligent mechanics in working out maritime improvement, whereby they would render important service both to themselves and their country.

He had now to make a few remarks upon the seaworthiness of the Monitors. It had been argued that the Monitor was such a precarious species of vessel that its action in the water might be compared with a hydrometer, which would bob up and down in the water with great ease, till there was danger of its disappearing altogether. He was not aware that a hydrometer was made broad at the water-line so as to present great resistance to immersion. Not only were there excellent mechanical reasons for believing the Monitors were good sea-boats, but they had the fact vouched by unimpeachable evidence from all parts of the world. It was said, first of all, Monitors might be useful for harbour purposes, but they could never be sent to sea. Well, they did go to sea, and on the occasion of the heavy gale just before the attack on Fort Fisher, the Monitors were the only vessels that did not drag their anchors. Next it was said Monitors might do for purposes at home, but they could not be sent on distant voyages; but one did go a voyage round Cape Horn. Lastly, it was said the voyage round Cape Horn was only a coasting voyage, and that the Monitors could not cross the Atlantic. Well, a Monitor had crossed the Atlantic and visited this country! It was further argued that in rough water, with waves 6 feet high over the deck Monitors could not use their guns, whereas the broadside ship could. Now, in the first place, the assumption that waves 6 feet high would run over the deck of a Monitor was gratuitous, the fact being that much less water came on board than might have been expected; and the commanders of the Monitors uniformly reported, that those vessels were able to use their guns under circumstances in which the broadside vessel could not open her ports at all. When a wave encountered the tall side of a broadside ship, the ship rose upon the wave by virtue of its own momentum; whereas in the case of the low sides of the Monitors the wave was spilt in a thin sheet, like a wave upon the sea shore, and the consequence was there was no rising of the water which prevented the working of the guns. On this point the report of Captain Fox respecting the performances of the 'Miantonomah' in the Atlantic must be accepted as conclusive, and he would read an extract from that report:—

> "The facts with regard to the behaviour of this vessel in a moderate gale of wind and a heavy sea, when a frigate would find it impossible to use her battery, are as follows:—With head to the sea, she takes over about 4 feet of solid water, which is broken as it sweeps along the deck, and, after reaching the turret, is too much spent to prevent the firing of the 15-inch guns directly ahead. With broadside to the sea, either when at rest or while moving, her lee-guns can always be worked without difficulty—the water which passes along the deck from windward being divided by the turrets, and her extreme roll so moderate as not to press her lee-guns near the water. Lying in the same position, her 15-inch guns can be fired directly astern without interference from water; and when stern to the sea, the water which comes on board is broken up in the same manner as when going head to it. . . . . The extreme lurch, when lying broadside to the sea in a moderate gale, was 7° to windward and 4° to leeward—average 5½°; while the average roll at the same time of the 'Augusta,' a remarkably steady ship, was 18°, and of the 'Ashnelot' 25°, both vessels being steadied by sail. A vessel which attacks a Monitor in a sea-way must approach very close to have any chance of hitting such a low hull, and even then the Monitor is half the time covered by 3 feet or 4 feet of water, protecting her and disturbing her opponent's fire. From these facts, not unknown to Monitor men, and the experience we derived from the use of such vessels during the war, we may safely conclude that the Monitor type of iron clads is superior to the broadside, not only for fighting purposes at sea, but also for cruising. A properly constructed Monitor, possessing all the requirements of a cruiser, ought to be constructed of iron, and have but one turret, armed with not less than 20-inch guns, two independent propellers, and the usual proportion of sail."

He now proposed to make a few remarks upon the "improved design" for Monitors submitted by Mr. Reed, as he thought the subject was of sufficient interest to justify him in so doing. He thought no competent observer could deny, that this design exhibited incontestable marks of superiority over everything before projected in this country; but it made too much concession to nautical prejudices, and therefore the grand principle of the Monitor system—concentration—was less perfectly developed than might otherwise be the case.

It had been stated in the discussion, that the fire of the Monitors was confessedly slow, and therefore it was advisable to have two turrets instead of only one, the turrets to be placed at a distance of 120 feet from each other, with a row of broadside guns between, protected by suitable armour; and all this it was said could be done with little increase in the weight of the ship. He thought it high time to get rid of such delusions. Every square foot of armour had its determinate weight, and with only a given displacement multiplication was only another name for weakness. Suppose the 'Dictator' were "improved" in the manner suggested, and instead of having one 15-inch turret it had two $7\frac{1}{2}$-inch turrets, each carrying two guns of half the weight, and sufficient metal stripped off the sides to form and protect a row of broadside guns, every one must feel that, instead of being a formidable man-of-war, it would be an emasculated weakling, wholly unable to deal with the circumstances of the present time, to say nothing of those which were impending. This vessel of Mr. Reed's seemed to stand in the same position as the supposititious 'Dictator' relatively with another vessel of the same size but greater concentration, when assailed by the great guns which Mr. Longridge and others might be expected to produce; and therefore, he said, reduce the height of the side to a minimum, roll the two turrets into one, set the pilot-house on the top of the turret, and gain rapidity of discharge, not by multiplying the guns, but by firing more shots from each gun. In other words, bring the type of the vessel as near that of the 'Dictator' as possible, only stronger and larger, and put solid armour on her. The changes proposed by Mr. Reed were, in his judgment, neither improvements nor novelties, for similar arrangements had been tried in America and been given up. There Monitors had been constructed with 3, 4, and 5-feet sides and two or more turrets, and in the original 'Monitor' the pilot-house was placed forward, communicating by speaking-tubes with the turret; but in action the inconvenience and confusion caused by this arrangement were found to be so great, that it was abandoned in favour of the section of the 'Dictator,' and no fault in that arrangement had been discovered. The escape of the 'Merrimack' in the encounter with the first Monitor was in a great measure due to the wrong position of the pilot-house forward of the turret, the error of which was then first found out. He did not suppose he had said anything which Mr. Reed did not already know, but he would press this reflection, that whatever propriety there was in deferring to nautical prejudices when Monitors were new and untried, there was no propriety in doing so now, when the sea-going qualities of these vessels had been proved.

According to Captain Hamilton the Monitors were of no use. They would neither shoot, nor go, nor resist, and were a ridiculous failure in every respect. They had not been told, if this were so, how it came to pass that the American Government, beginning with a single Monitor, had gone on building them till they had now seventy Monitors, nearly the whole of their iron-clad fleet being composed of vessels of that character. Nor had they been told how it happened, that a vessel propelled by given engine-power should go slower when laden with a few hundred tons of Monitor armour than when laden with a like weight of any other cargo. Then, what was to prevent a gun, if set on the same carriage and handled by the same men, from being fired as fast and as well from one kind of ship as from another? Would any one presume to say that a gun-vessel, with any given displacement, of which the area of the armour was made a minimum, and its thickness consequently a maximum, and in which the guns were similarly concentrated, must not be the strongest vessel, both to penetrate and to resist, that could be constructed on such a displacement? He had made personal inquiries of the officers of the Monitors, and their statements were diametrically opposed to the views expressed by Captain Hamilton. He found the Monitors, on an average, had been each twenty-five times in action during the war, being a larger amount of service than had been performed by any other war-vessels within the limits of history. He found that they were exposed to the most powerful ordnance which the Confederates could procure in England and elsewhere, without having been once pierced, that he was aware of; but if penetration did occur, it did not show that the Monitor system was bad, but only that in a particular case it was not carried sufficiently far to meet the great power of the guns arrayed against it. From a list of the guns in the American forts with which the Monitors contended, he found that they comprehended 13-inch rifled Blakeley, 11-inch Dahlgren, 10-inch rifled Columbiads, and many more, making one hundred and forty-three guns in all. The circumstances connected with the construction of the first Monitor were of a most dramatic character. Nevertheless, it must not be supposed that the Monitor system had obtained its present prominence without challenge or opposition, or without much objection, disparagement, and misapprehension. And if, after adequate experience, it had surmounted all this, and had been definitely adopted in America as the backbone of the navy, it could only have been because it was found to be more effective than all other systems of which there had been previous knowledge. Mechanically it was the only sound system. Practically the people who had most tested it were the most wedded to its use. He knew all about Admiral DuPont's objections, and was able to explain them. But they were personal, and had nothing to do with the system itself.

On reviewing the course of this discussion, he thought it was generally agreed that in smooth water, or with small waves, vessels of the Monitor type were the most powerful war-vessels that could be employed, and were indispensable; but that the doubt still existed how far they were all that could be desired at sea. How were these doubts to be finally resolved except by practically testing a vessel? There would not be much risk in the experiment, as if the vessel was unsuitable for one purpose it would be invaluable for another. It appeared to him in the introduction of the Monitor system into the Royal Navy, Mr. Ericsson's aid would be most important, because he, of all men, had best tested it in actual war, and would be able to tell, without serving a new apprenticeship, what particular arrangements were best, whereby the highest measure of efficiency could be secured, and failure or imperfection be prevented. He had no doubt Mr. Reed, if free to speak his mind, would express his sense of the vast importance of such a cooperation.

In conclusion, he would only ask what was the verdict of the Institution in this important question? For he considered his audience more competent than any other that could be collected elsewhere to pronounce judgment upon it; and that judgment ought to sway the national councils. Was it in favour of Monitors or not? And, if favourable, how could a Monitor fleet be better constructed than from designs of Captain Ericsson and Mr. Reed conjointly?

Mr. Gregory, V. P., remarked that the rules of the Institution would not allow a verdict being given on the subject of Monitors; but if such a course were consistent with the rules, he doubted if Mr. Bourne's eloquence would convince the Members, that this country ought to incur great expense in the construction of Monitors, without very carefully weighing the experience that had been gained in America; and, while he did not wish to prejudge the question, he thought that the mishaps which had attended their use in that country, might fairly be urged against their adoption to any great extent by this country. Looking to the length to which the discussion had extended, he would not attempt to enter into any of the details of Mr. Bourne's interesting Paper, or of the observations to which it had given rise, but he could not help remarking, that gentlemen of great practical experience and high scientific attainments came to most contradictory conclusions with regard to the construction, protection, and armament of Ships of War. With this fact in view, he thought some little indulgence might be extended to the officials of the Government, if they had not moved as quickly as some sanguine people desired. There still remained one great question unsolved, viz.: the struggle between the resisting power of armour plating, and the penetrating power of guns. At present the balance of power appeared to be in favour of guns; but while there were men of such talent engaged in that struggle, it would be bold for any one to predict the ultimate result. At the same time, if armour plating was to be increased in thickness to meet the continually increasing power of guns, he thought it would be difficult to maintain, in ships so loaded, many qualities which were essential to their successful use; and in this view he could not but fancy, that the time might come when armour-plating in the navy, if not a thing of the past, might become of partial or exceptional use.

January 22 and 29, and February 5, 1867.

CHARLES HUTTON GREGORY, Vice-President, in the Chair.

The discussion upon the Paper, No. 1,163, "Ships of War," was continued throughout the Meeting, to the exclusion of any other subject.

At the Meeting of February 5th, the following Candidates were balloted for and duly elected :—Charles Bernard Baker, Henry Baylis, Michael Beazeley, James Bolland, William Crozier, James Deas, Thomas Fenwick, Daniel Gallagher Grose, John William Grover, Henry Law, William Lawford, George Owen, Charles Robins, Francis Stevenson, and Thomas Jefferson Thompson, as Members; William Henry Ashwell, Joseph Parkin Colbron, Vice-Admiral George Elliot, William Francis, John Clarke Hawkshaw, B.A., Follett Charles Hennet, Harry Pasley Higginson, John Howkins, jun., George Albert Hutchins, Charles Edward Mackintosh, John James Myres, jun., Charles O'Neill, Robert Robertson, Captain R.N., Alexander Clunes Sherriff, M.P., Henry Thomas Tanner, Charles Brown Trollope, and William Cawthorne Unwin, B.S., as Associates.

February 12, 1867.

JOHN FOWLER, President,
in the Chair.

No. 1,156.—"Description of the Clifton Suspension Bridge."[1] By William Henry Barlow, M. Inst. C.E., F.R.S.

In giving an account of the Clifton Suspension Bridge, it is desirable, before proceeding to describe its mechanical details, to say a few words upon the history and origin of the undertaking.

In the year 1753, William Vick, a highly-respected Alderman of Bristol, bequeathed the sum of £1,000, which by his will was to be placed in the hands of the Merchant Venturers of Bristol, to accumulate at compound interest until it reached £10,000, and was then to be used in constructing a stone bridge at or about the site of the present Clifton Bridge. To the money so furnished were added contributions and subscriptions from numerous persons in Bristol and its vicinity; and in 1830 an Act of Parliament was obtained for the construction of the bridge, to which the late Thomas Telford was Engineer. In this Act, power was taken to make the bridge of iron and stone; and the design of Telford, which accompanies the estimate deposited in the Private Bill Office, shows a suspension bridge, having two very tall stone piers, rising from the river at just sufficient distance apart as not to interfere with the river or the roadways on each side of it. The bridge consisted of three spans, of which the centre span was thus made under 400 feet.

The work subsequently passed into the hands of the late Isambard Kingdom Brunel, who made a new design, placing the piers near the top of the rocks on each side, and boldly crossing the whole opening in one span of 702 feet, at a height of nearly 250 feet above high water. This design was proceeded with in 1836 and the following year, so far as to complete the abutments and erect the piers ready to receive the saddles for supporting the chains. The work, however, was impeded from insufficiency of funds, and ceased altogether in 1843, from which time, until 1860, no further progress was made.

At that time the Charing Cross Railway in London was being constructed, under the direction of Mr. Hawkshaw, and, it having been decided to erect a girder bridge at Hungerford, instead of the then existing suspension bridge, the chains of the Hungerford Bridge were set at liberty, and the opportunity thus afforded, of obtaining good chains at a comparatively low cost, available for the Clifton Bridge, was immediately taken advantage of; Mr. Hawkshaw and the Author becoming joint Engineers to the revised project, under whose designs and direction the work has been brought to a successful issue.

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[1] The discussion upon this and the following Paper occupied portions of three evenings, but an abstract of the whole is given consecutively.

A Company was formed for the completion of the bridge, which was warmly taken up by the personal friends of Mr. Hawkshaw and the Author. Considerable local support was given to the undertaking, and, assisted by other gentlemen of influence and position, the Company commenced its operations. Arrangements were entered into for the purchase of the land and piers at Clifton, and of the chains from Hungerford, and application was made to Parliament for the requisite powers to complete the bridge. The Act was obtained in 1861, and the works were commenced as soon as the chains were obtained from Hungerford.

In adapting the chains of the Hungerford Bridge to Clifton, it became necessary to vary and re-arrange the design of the superstructure of the Clifton Bridge, as contemplated by Mr. Brunel. In that design only two chains were proposed, carrying a roadway formed of trussed woodwork, and stiffened by an arrangement of vertical struts, with diagonal tie-bars of iron.

In the Clifton Bridge as executed, Plate 10, Fig. 1, there are three chains on each side, supporting longitudinal stiffening girders of wrought iron, with open-work cross girders, the hand-railing of the bridge being made also to form longitudinal stiffening girders with open-work sides.

The principal dimensions of the bridge are: Span, 702 feet 3 inches; distance from centre to centre of chains, 20 feet; width of bridge, including roadway and footways, 31 feet; versed sine of curve of chains, 70 feet; height of roadway above high water, 248 feet. The roadway has a curve upwards, or camber, of 2 feet between the piers.

The chains are carried upon the piers by wrought-iron saddles, placed on roller-frames of cast iron, the rollers being made of cast steel. The beds of the roller-frames are at an inclination of 1 in 20, rising towards the river. At a distance of 196 feet from the centres of the piers, land-saddles are placed, which are similar in construction to the saddles upon the piers, except that they have no roller-frames, but are bedded upon brickwork in cement set upon the solid rock. From the land-saddle to the anchorage is a distance of 60 feet, with an average inclination of 45°. In this distance the three chains gradually diverge, until they become 5 feet apart, where they are inserted through the castings which form the anchorage-plates. Each chain has a separate anchorage-plate, 5 feet by 6 feet, bedded upon a mass of brickwork set in cement, built in plan in the form of an arch, abutting upon the solid rock.

The length of the links in the centre of the bridge, where they are horizontal, is 24 feet, increasing in length as their angles of inclination increase. The three chains are in such a relative position to each other as to produce an equal horizontal distance of nearly 8 feet from the centre of the suspension-rods throughout the bridge.

The links in the Hungerford Bridge, Plate 10, Fig. 4, were arranged so that the joints of the links in the upper chain were opposite to the middle of the links of the under chains; and the suspension-rods were carried by a jointed link, in such manner that half the weight was borne at the point of the one chain, and the other half by the middle of the links of the other chain. In this mode of hanging the suspension-rods, a transverse strain of considerable amount was brought upon the middles of the links of the chains.

In the Clifton Bridge this has been avoided. The suspension-rods transmit their strain to the chains only at the joints. The result is, that the links of the chain have no other strain upon them than that of the direct tensile strain in the direction of their length. The duty of maintaining an equal action upon all three chains in supporting the roadway is performed by the strength and stiffness of the longitudinal girders. The suspension-rods are attached to the longitudinal girders, in the manner shown by the diagram, Plate 10, Fig. 2, each rod being furnished with a double adjusting screw at the lower extremity.

All the links of the Hungerford Bridge were tested to a strain of 10 tons per square inch before being erected in that bridge, and all the new links required to complete the third chain at Clifton have also been tested with a strain of 10 tons per inch. The diameter of the bolts which connect the links together is $4\frac{5}{8}$ inches.

The roadway is formed of a deck of 5-inch creosoted Memel planking, grooved and tongued with iron. Upon this is placed a second coating of planking transversely, which forms the wearing surface, and may be changed or varied in construction, as the exigencies of the traffic may require. The footways are formed of $2\frac{1}{2}$-inch planking, laid upon longitudinal bearers.

The execution of this work was intrusted to the firm of Messrs. Cochrane, who were also the Contractors for the iron-work of the new Hungerford, or Charing Cross, Bridge, and for the removal of the former Suspension Bridge at that place.

The general arrangements made by them, in conjunction with the Engineers, for the erection of the chains, were as follows:—

A temporary suspended staging was constructed, of eight iron wire ropes, each rope being capable of bearing 35 tons. (Plate 10, Fig. 3.) Six ropes were placed beneath the planking, and two at a height of 3 feet 6 inches above, the latter serving to form a hand-railing on each side. The upper ropes were attached to those below by hoop iron doubled over, and riveted, of sufficient strength to cause the upper ropes to act in conjunction with the ropes beneath the planking in sustaining any weight placed upon the staging. Above this staging another rope was fixed, for the purpose of carrying two light travelling frames suspended on wheels, which were moved as required by light ropes, and by means of which, links were taken from the piers to the men engaged in erecting the chain.

The chains were commenced at the anchorage-plates at each end simultaneously—the lowest chain being put in first. At the anchorage-plates the whole of the links, twelve in number, were inserted; then eleven, ten, nine, eight, and so on until the chain was diminished to one link; after which it was continued at one link and two links alternately from the piers, until it met in the middle of the centre opening.

The wire-rope staging was designed to carry the weight of the centre portions of the chain, formed of one and two links alternately, with the men and tools required to erect it. The calculated breaking-weight of this staging was 224 tons, evenly distributed.

The weight it had to carry was 40 tons:—

	Tons.
Chain formed of one and two links alternately . .	12
Workmen and tools (say)	5
Planking	10
Wire ropes	8
Bolts, nuts, and sundries, including packing for links	5

or less than $\frac{1}{5}$th of the breaking-weight.

The suspended platform was kept below the intended level of the chain, and the links were supported upon it by packing pieces, which could be raised or lowered. When the links of the chain were united in the middle, the packing pieces upon the staging were lowered until the chain took its own bearing, and thus relieved the staging from the action of its weight. At this stage of the proceedings the chain was adjusted for length, by means of keys arranged for that purpose in the first links from the pier saddles.

The next operation was that of adding links on each side of the centre links, which was rapidly accomplished by an ingenious and simple apparatus, contrived by the Contractors and their able manager, Mr. Airey. The rapidity with which the side links were added depended much upon the state of the weather; but there were some days on which more than one hundred links were added to the chain.

In connection with the temporary staging, there were many other pieces of apparatus or contrivances for winding, tightening, and fastening, which were excellently carried out.

The chains on the Bristol, or eastern, side of the bridge having been completed, the staging was shifted over to the other side, and the remaining three chains were put up in like manner.

When the chains were complete, and the suspension-links for carrying the suspension-rods were fixed, the next operation was that of attaching the suspension-rods and cross-girders. This was accomplished by an apparatus made by the Contractors for the purpose, consisting of a moveable crane, upon a long base frame, weighing rather over 5 tons, and travelling upon a temporary railway. It was so contrived and balanced that it could carry the weight of a cross-girder (with a portion of the longitudinal girders attached) at a considerable distance in advance of the wheels upon which it travelled. Thus, when placed upon the abutment, it held the first cross-girder in its intended position until the men attached the first pair of suspension-rods to it and to the chain. Planking was then laid from the abutment to the cross-girder, and the railway was lengthened. The travelling-crane then took up the second cross-girder, and advanced with that to its position, and held it in like manner until it was attached to the chain. The planking and roadway were again lengthened, and the third cross-girder fixed, and so on from both ends of the bridge, until the roadway met in the middle.

The remaining operations of connecting the longitudinal girders, adjusting the several parts of the work, and laying the roadway, were then proceeded with.

The manner in which the weights were of necessity brought upon the chain, commencing as they did from the abutments and proceeding towards the centre of the opening, caused the curve of the chain to vary in figure very much during the construction; but the ultimate curve, which resulted when the roadway was completed, accords with remarkable accuracy to the calculated curve, and the calculated lengths of the several suspension-rods.

The sectional area of the chains at the piers is 481 square inches, and in the centre of the span 440 square inches. The weight of the chains between the piers is 554 tons. The strain produced at the centre of the chains by the weight of the chains themselves is nearly 680 tons. The weight of the suspension-rods, longitudinal girders, transverse girders, cross bracing, hand railing, roadway, &c., is about 440 tons. This weight is not spread over the whole of the 702 feet span, but occurs between the abutments which project in front of the piers, leaving a space between them of 636 feet. The distance of the centre of gravity of the half span of the platform from the centre of the pier is 190 feet nearly. The strain produced by the weight of the platform at the centre of the chains is 597 tons approximately. The maximum moving load, estimated at 70 lbs. per square foot, or 600 tons upon the platform of the bridge, would produce a strain of 817 tons at the centre of the chain.

The total strain at the centre would, therefore, be as follows:—

	Tons.
Strain due to the chains	680
,, ,, to the weight of the platform rods, &c.	597
,, ,, to the load of 70 lbs. per square foot upon the platform	817
Total. .	2,094

To carry this strain there are 440 square inches of metal, so that the maximum strain upon the iron is $\frac{2094}{440} = 4{\cdot}76$ tons per square inch; of which the strain produced by the bridge itself is 2·90 tons per square inch.

It is to be observed that the piers, although the same height above the abutments, are not level with each other. The pier on the Leigh Wood side is 3 feet lower than that on the Clifton side, and the whole bridge has an average inclination of 1 in 233. This inclination was given to the structure by Mr. Brunel to obviate what he thought would have been an ocular deception (had the bridge been horizontal), arising from the difference in the height and form of the rocks on the two sides of the river. This peculiar arrangement makes a difference in the strains upon the chains in the two halves of the bridge, and in the form of the curve of the chain, which had to be calculated and provided for in the lengths of the suspension-rods. The calculations for the form of the curve, and the lengths of the respective parts, were made by Mr. Alfred Langley, formerly a pupil, and now an assistant, of the Author; and it is satisfactory to add, that there was not a single rod or part of the bridge which had to be altered in execution.

The suspension-rods are each rather more than 2 inches in section. The greatest weight that can come upon a pair of rods, including their maximum load, is about 13 tons, which would produce a strain of 4¼ tons per square inch.

The brickwork employed in the anchorage and land-saddles is of Staffordshire blue bricks, laid in Portland cement, and the anchorage-plates and the bearing-plates of the saddles are so arranged that the maximum pressure upon the brickwork cannot in any case exceed 10 tons per square foot.

The bridge is stiffened longitudinally by two plate-girders, 3 feet deep, placed on each side of the roadway, the upper and lower flanges of these girders being 11 inches in sectional area. The hand-railing, which is in effect a lattice-girder, 4 feet 9 inches deep, with upper and lower flanges of 4½ inches sectional area, also adds to the stiffness of the bridge. Both these girders act in giving stiffness against a horizontal transverse strain, and are assisted in that action by a system of diagonal bracing throughout the bridge, formed of bars 4 inches by ½ an inch in section placed beneath the roadway.

In order to provide for the effects of expansion and contraction, and to allow for the movement occasioned by wind and by the passage of heavy loads across the bridge, the two extremities of the roadway are furnished with jointed ends or flaps, 8 feet long, which give perfect freedom of motion both vertically and in the direction of the length of the bridge.

The whole of the arrangements for executing this work have been carried out in the most able manner by the Contractors, Messrs. Cochrane, and by their resident manager, Mr. Airey. The works were commenced at Clifton in November, 1862, and the bridge was opened for public traffic on December 8th, 1864. The total cost of the ironwork, including the purchase of the chains from Hungerford and their carriage to Clifton, was £34,975. Previous to opening the bridge for public traffic it was tested by a dead weight of 500 tons of stone distributed over the surface; and was inspected during the test by Mr. Charles Manby, acting on behalf of the President of The Institution of Civil Engineers, Mr. M'Clean, who was abroad at the time. The total deflection produced by the test-load was 7 inches in the centre of the bridge, which deflection resulted not only from the elongation of the metal due to the stress, but also from the altered position of the saddles upon the piers, arising from the diminished curvature of the land-chains. When the test-weight was removed, the centre of the bridge rose again to its former position within one-sixteenth of an inch, but the middle of the southern half of the bridge did not rise again to its former height by one inch, while the northern side rose above its original position. This circumstance was probably due to the change in the direction and force of the wind before and after the testing.

The following is a tabular statement (p. 250) of the results of the testing above referred to:—

Since the completion of the bridge it has had to sustain large and irregular-moving loads. Being a work of great local interest, it has on several occasions been crowded with people. It is evident, however, that the most severe strain which it will have to resist is that resulting from heavy gales of wind. South-westerly gales blow

CLIFTON SUSPENSION BRIDGE.

TABLE showing the Deflections produced by the Test Load of 500 Tons evenly distributed.

South Chain.					North Station.				
Number of Suspension Rod.	Level before the Load was put on.	Level with the full Load of 500 Tons.	Level after the Load was removed.	Permanent Set.	Number of Suspension-rod.	Level before the Load was put on.	Level with the full Load of 500 Tons.	Level after the Load was removed.	Permanent Set.
	Feet.	Feet.	Feet.			Feet.	Feet.	Feet.	
Leigh Wood side 1	4·77	4·73	4·76	−·01	Leigh Wood side 1	4·78	4·75	4·77	−·01
6	3·94	3·98	3·95	+·01	6	3·90	3·95	3·88	−·02
11	3·33	3·48	3·37	+·04	11	3·28	3·44	3·31	+·03
16	2·75	3·02	2·83	+·08	16	2·72	3·00	2·76	+·04
21	2·22	2·62	2·28	+·06	21	2·18	2·58	2·26	+·08
26	1·75	2·24	1·83	+·08	26	1·66	2·18	1·75	+·09
31	1·40	1·97	1·46	+·06	31	1·25	1·82	1·33	+·08
36	1·10	1·72	1·15	+·05	36	0·92	1·55	0·98	+·06
41	0·92	1·54	0·94	+·02	41	0·75	1·35	0·75	·00
				Centre rod.					Centre rod.
41	4·00	4·59	4·01	+·01	41	3·82	4·40	3·83	+·01
46	3·90	4·48	3·93	+·03	46	3·70	4·28	3·71	+·01
51	3·90	4·40	3·90	·00	51	3·72	4·21	3·73	+·01
56	3·92	4·33	3·93	+·01	56	3·75	4·13	3·75	·00
61	3·98	4·31	4·02	+·04	61	3·83	4·13	3·84	+·01
66	4·10	4·34	4·15	+·05	66	4·02	4·21	4·02	·00
71	4·32	4·44	4·32	·00	71	4·22	4·31	4·21	−·01
76	4·50	4·51	4·49	−·01	76	4·50	4·47	4·44	−·06
Clifton side : 81	4·90	4·84	4·88	−·02	Clifton side . 81	4·95	4·83	4·89	−·06
		Saddles on piers travelled forward, $1\frac{1}{8}''$.	Saddles travelled back again, $\frac{15}{16}''$.				Saddles on piers travelled forward, $1\frac{1}{8}''$.	Saddles travelled back again, $\frac{15}{16}''$.	

nearly in the direction of the length of the bridge, and the high ground on either side diminishes their effect upon the structure. But gales from the north-west or south-east, being nearly in the direction of the deep gorge of the River Avon at the place where the bridge is constructed, impinge upon the work with great violence, so much so that it is difficult at times to stand upon the roadway. On these occasions three effects are observed:

First, there is a small horizontal deflection, which is just sufficient to be perceptible to the eye when placed in range with the suspension-rods. Secondly, there is an undulation from end to end of the bridge. It is a slow and stately movement of the structure, which manifests itself by a rising and falling of the roadway about half way between the centre and the abutments, alternating from the north-eastern to the south-western portion of the bridge. Mr. Airey, being present on more than one occasion when the bridge was subjected to this kind of action, endeavoured to ascertain its amount; but it is difficult to measure the action, as the violence of the wind prevents the use of any ordinary measuring instruments. As far as he was able to judge, the maximum rise and fall was 6 inches above and 6 inches below the mean level of the roadway. The third effect produced by wind is the motion imparted to the land-chains. It will be observed that there are no suspension-rods between the piers and the land-saddles, so that the chains hang without anything to restrain their motion. Violent gusts of wind are capable of deflecting these chains laterally, notwithstanding their weight, the longitudinal strain upon them, and the comparatively small surface exposed to the wind.

Two features of this bridge appear worthy of remark. One is the facility with which the work was executed; the other is the comparatively inexpensive nature of the scaffolding, or temporary staging, required for erecting the chains, considering the distance between the points of support. Both these results are due and belong to the principle of suspension; and there appears no reason why spans of much greater magnitude should not be accomplished by similar means with equal certainty. Already spans crossed by bridges on the principle of suspension far exceed those of any form of girder. The largest girder ever built is that of the Britannia Bridge, 460 feet between the bearings. The largest suspension bridge is that at Fribourg, which is stated to be 880 feet. There is also the Niagara Bridge, of 820 feet from centre to centre of the towers, which has been in daily use for nearly twelve years for the passage of railway trains.

To make a detached iron girder capable of carrying heavy weights over a span exceeding 800 feet would involve such an excessive weight of metal that it may be pronounced impracticable, and therefore when such spans are to be dealt with, recourse must be had to some other principle of construction.

Suspension bridges have not hitherto been adopted in this country for railway purposes, under the impression that the principle of construction necessarily involved such an amount of flexibility as to render them unfit for the passage of trains; but it must be considered that the larger the bridge, and the greater the ratio of the weight of the bridge to the weight of the moving load, the less will be the disturbance of form caused by a passing load. Moreover, it is quite practicable to stiffen a suspension bridge so as to render it nearly as rigid as a girder. Of this the Lambeth Bridge, from the design of Mr. P. W. Barlow, M. Inst. C. E., affords an example. The stiffening is accomplished by means of diagonal ties and vertical struts, and is perfectly successful. The Lambeth Bridge, whether as regards its action when exposed to heavy gales of wind or to a passing load, possesses great rigidity, a result which is the more remarkable as the main supports of the structure are wire ropes.

Another design, having for its object to impart rigidity to a suspension bridge, is that of Messrs. Ordish and Le Feuvre. The arrangement is highly ingenious, but, as at present applied, it is not available in a series of spans.

The subject of stiffening suspension bridges with the least quantity of metal is one well deserving attention. It has been stated, that if a chain were so stiffened as to be as rigid as a girder, it would involve the use of as much metal as a girder; but a little consideration will show that the conditions of the two structures are entirely different. In a detached girder the upper and lower booms must each be capable of bearing the strains produced by the weight of the bridge and its load, and the diagonals must be strong enough to transmit the whole of these strains; whereas, in a stiffened suspension bridge, the chain is the only member required to bear the strains produced by the weight of the bridge and its load, while the diagonal bracing or stiffening need be no more than is sufficient to prevent disturbance from the moving load.

In relation to this subject it may be right to mention a construction of continuous girder which approaches (in regard to economy of materials employed) to a stiffened suspension bridge. In a continuous girder, if, instead of using an equal depth throughout, a greater depth and a greater sectional area be given over the piers, it has been found by experiments on solid bars, as well as from a theoretical investigation of the case as applied to lattice girders, that an increase of strength is obtained in a much higher ratio than that of the increased weight of metal employed.

The experiments on the bars referred to were made by the Author in 1858, and the material employed was cast iron, which was selected because the relative strengths of the different forms were indicated in a distinct and decided manner, by the actual rupture of the beam. The experiments extended to a comparison of strengths and stiffness of four descriptions of bars. (Plate 10.)

1st. Detached bars, parallel throughout.
2nd. Continuous bars, of equal section throughout.
3rd. Parallel continuous bars, in which the sectional area was increased over the piers without increasing the depths.
4th. Continuous bars, in which both the sectional area and the depth were increased over the piers.

The continuous bars were tried under three separate conditions of loading:—

1st. The centre span alone was loaded.
2nd. The load on the centre span per unit of length was made double the load in the side spans.
3rd. The load was evenly distributed throughout the whole length of the bar.

The detached bar was 6 feet between the bearings: the continuous bars were 15 feet long, having a centre opening of 6 feet, and two side openings, each 4 feet 6 inches.

The results are given in the following Tables (pp. 254, 255):—

In the last form, where the area and the depth are each made double over the pier, while the bar is increased in weight only 25 per cent. in the centre opening, the strength and stiffness are increased in a much higher proportion.

The mean relative strengths of this bar, in the three conditions of loading, that of the detached bar being taken as 1, were as follows:—

When loaded only in the centre span . . .	2·60
When the load per foot on the centre span was twice that of the side spans	3·93
When the load was equally distributed throughout the bar	4·42

The stiffness was increased in a still higher proportion, for, taking the deflection of a detached beam at 100, the deflections of the continuous beams under the three conditions of loading, were 31, 21, and 7, respectively.

Table of Experiments on Cast-iron Beams, showing the Weight on the Centre Span at the Time of Fracture.

Condition of Loading.	No. 1. Uniform Continuous Beam, 1 inch square, 15 feet long, supported at the ends, and on piers 4 feet 6 inches on each side the centre.	lbs.	No. 2. A similar Beam, increased three times in width over the piers, the weight of Beam being increased 50 per cent.	lbs.	No. 3. A similar Beam increased to double the depth over the piers, the weight of Beam being increased 25 per cent.	lbs.
Loaded only on centre span, the ends being fastened down.	Experiment No. 1	1,032·0	Experiment No. 1	1,567·2	Experiment No. 1	1,569·6
	„ „ 2	1,254·4	„ „ 2	1,656·0	„ „ 2	1,836·0
	„ „ 3	1,186·4	„ „ 3	1,567·2	„ „ 3	1,636·2
	„ „ 4	1,054·2	„ „ 4	..	„ „ 4	1,658·4
	Mean . .	1,131·75	Mean . .	1,596·8	Mean . .	1,675·05
The load per foot on centre span being twice that on side spans.	Experiment No. 1	1,411·4	Experiment No. 1	1,882·4	Experiment No. 1	2,062·4
	„ „ 2	1,637·6	„ „ 2	2,107·2	„ „ 2	2,712·8
	„ „ 3	1,433·6	„ „ 3	1,726·4	„ „ 3	2,916·8
	„ „ 4	1,411·4	„ „ 4	1,949·0	„ „ 4	2,420·0
	Mean . .	1,473·5	Mean . .	1,916·25	Mean . .	2,528·0
Beam loaded uniformly over its whole length.	Experiment No. 1	1,413·4	Experiment No. 1	2,465·6	Experiment No. 1	2,556·8
	„ „ 2	1,300·8	„ „ 2	2,284·8	„ „ 2	3,212·4
	„ „ 3	1,233·6	„ „ 3	1,972·8	„ „ 3	2,510·4
	„ „ 4	1,277·6	„ „ 4	2,356·0	„ „ 4	3,094·4
	Mean . .	1,306·35	Mean . .	2,269·8	Mean . .	2,843·5

Detached Beam, 1 inch square, loaded uniformly. Length of bearing 6 feet, equal to centre span of Beams, Nos. 1, 2, 3.	lbs.
Experiment No. 1 . .	696·0
„ „ 2 . .	717·6
„ „ 3 . .	662·1
„ „ 4 . .	494·4
Mean . .	642·52

Summary showing the Breaking Weights as compared with a detached Beam.

Condition of Loading.	No. of Beam.	Mean relative Strength of each Form of Beam, that of Detached Beam being 1.
Loaded only on centre span, the end being fastened down	1	1·7614
	2	2·4853
	3	2·6070
The load per foot on centre span being twice that on side spans	1	2·1967
	2	2·9824
	3	3·9346
Beam loaded uniformly over its whole length . .	1	2·0328
	2	3·5327
	3	4·4256

TABLE showing the DEFLECTIONS with 100 lbs. uniformly Distributed over CENTRE SPAN.

Condition of Loading.	No. 1. Uniform Continuous Beam.		No. 2. Continuous Beam, increased three times in width over the Piers, the weight of Beam being increased 50 per cent.		No. 3. Continuous Beam increased to double the depth over the Piers, the weight of Beam being increased 25 per cent.	
		inch.		inch.		inch.
Loaded only on centre span, the ends being fastened down.	Experiment No. 1 .	·1703	Experiment No. 1 .	·1308	Experiment No. 1 .	·1114
	,, ,, 2 .	·1572	,, ,, 2 .	·1274	,, ,, 2 .	·1090
	,, ,, 3 .	·1501	,, ,, 3 .	·1381	,, ,, 3 .	·1066
	,, ,, 4 .	·1669			,, ,, 4 .	·1187
	Mean . . .	·1611	Mean . . .	·1321	Mean . . .	·1114
		inch.		inch.		inch.
The load per foot on centre span being twice that on side spans.	Experiment No. 1 .	·1428	Experiment No. 1 .	·0979	Experiment No. 1 .	·0795
	,, ,, 2 .	·1317	,, ,, 2 .	·0935	,, ,, 2 .	·0788
	,, ,, 3 .	·1317	,, ,, 3 .	·0916	,, ,, 3 .	·0723
	,, ,, 4 .	·1246	,, ,, 4 .	·0955	,, ,, 4 .	·0723
	Mean . . .	·1327	Mean . . .	·0946	Mean . . .	·0757
		inch.		inch.		inch.
Beam loaded uniformly over its whole length.	Experiment No. 1 .	·0843	Experiment No. 1 .	·0496	Experiment No. 1 .	·0228
	,, ,, 2 .	·0823	,, ,, 2 .	·0477	,, ,, 2 .	·0250
	,, ,, 3 .	·0799	,, ,, 3 .	·0472	,, ,, 3 .	·0239
	,, ,, 4 .	·0799	,, ,, 4 .	·0497	,, ,, 4 .	·0283
	Mean . . .	·0816	Mean . . .	·0485	Mean . . .	·0250

Detached Beam loaded uniformly. Length of bearing equal to centre span of Beams, Nos. 1, 2, 3.	
	inch.
Experiment No. 1 . .	·3555
,, ,, 2 . .	·3296
,, ,, 3 . .	·3640
,, ,, 4 . .	·3505
Mean . .	·3499

Summary showing the Deflection as compared with a detached Beam.

Condition of Loading.	No. of Beam	Mean relative Deflections, that of detached Beam being 100.
Loaded only on centre span, the ends being fastened down	1	46·04
	2	37·75
	3	31·83
The load per foot on centre span being twice that on side spans	1	37·92
	2	27·04
	3	21·63
Beam loaded uniformly over its whole length . .	1	23·31
	2	13·86
	3	7·14

Another point well deserving of consideration is the best form of links and fastening for the chains of a suspension bridge. From experiments made by Sir Charles Fox, and published in the Proceedings of the Royal Society,[1] it appears that in order to develop the full strength of the metal in the centre portion of the link, it is necessary to make the pins of much larger diameter than has been customary. The employment of such large pins, together with the metal required in the enlarged ends of the links round the pins, adds an amount of dead weight to the chains which it is desirable to avoid or diminish; and it becomes an important question to inquire whether some better mode of connection can be found for the chains of suspension bridges.

But where the object is to construct bridges of large span, another, and perhaps the most important consideration, is the employment of a stronger material. In this respect the introduction of steel is calculated to have a marked influence. Many of the properties of steel are at present unknown. This want of knowledge produces a want of confidence in the employment of the material, and it would be a great boon to the profession, if steel were subjected to a like investigation as that made by the late Mr. Eaton Hodgkinson upon iron. Experiments made by Mr. Kirkaldy, and others made by the Author of this Paper with the testing-machine at the Royal Arsenal, Woolwich, have shown that the tensile strength of steel is nearly double that of wrought iron, and that it can be made quite as malleable. Its powers of resistance to compression do not show the same proportion of strength; but in the application of steel to the chains of a suspension bridge, it is the tensile strength which operates.

The introduction of a material of twice the tensile strength of iron would enable a bridge of twice the span of the Clifton Bridge to be made with the same sectional area in the chains (the ratio between the versed sine of the curve and the length of the chains being similar, and the weight of the roadway and load per foot the same in each case); whereas, to make a bridge of double that span in wrought iron would require nearly four times the section in the chains. The importance of so great a reduction in weight in crossing openings of large span can hardly be overrated. It operates not only in diminishing the actual quantity of material in the structure itself, and the consequent strains on the abutments or anchorages, but it diminishes very greatly the cost of the temporary staging and scaffolding required to construct the work; in fact, structures of great span would become practicable in steel which would be utterly impracticable in iron.

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[1] Vol. xiv. page 139.

In concluding this Paper, the Author desires to revert to one feature in the history of the Clifton Bridge which ought not to pass unnoticed. When, in 1860, the project for the completion of the bridge was once more brought under public notice, the commercial prospects of the undertaking were not of a nature to give any hope of an adequate return for the money invested, and it was, consequently, hopeless to apply to the usual sources to obtain the requisite amount of money for its construction. The promoters therefore brought the undertaking, in the first instance, under the notice of the leading Engineers and Contractors, not as a matter of profitable investment, but to ask their assistance in completing a great public work commenced by Brunel, to which much public interest was attached. This application was met in the most liberal spirit by many whose names did not appear in any way before the public. Not only were the actual contributions thus afforded an important element in the early stages of the Company, but the influence of the names of those gentlemen, in addition to those on the Provisional Committee, attracted an amount of interest and support which contributed greatly to the successful issue of the undertaking.

The Paper is illustrated by a series of diagrams, from which Plate 10 has been compiled.

February 19, 1867.

JOHN FOWLER, President,
in the Chair.

No. 1,175.—"On the Use of the Suspension Bridge with Stiffened Roadway, for Railway and other Bridges of great Span."[1] By GEORGE BIDDELL AIRY, Astronomer Royal, Hon. M. Inst. C.E.

THE idea which forms the basis of the following communication first presented itself to the Author on discussing with Mr. Robert Stephenson the plan to be adopted for the Britannia Bridge. Though it was not then fully matured, its general plausibility appeared sufficient to justify him in stating to Mr. Stephenson, that he thought there were better methods available for wide crossings than simple tubular bridges. No necessity for perfecting the theory arose for many years, although the knowledge of the general recognition, by Engineers, of the expense and difficulty of extending the tubular construction to dimensions much greater than those of the Britannia Bridge, induced the Author to bear its principles in mind.

About three years ago, the Author was permitted to examine the plans of a railway bridge of great span, then proposed; and, after making a counter-proposal of a plan on the principles of the present Paper, he proceeded to treat it by a mathematical process. No positive difficulty presented itself in this investigation; but the complexity of symbols became so great that, after several times recurring to it, he determined to refer to theory for considerations of a general class only, and to rely on experiment for the numerical determinations. These general theoretical considerations and experimental determinations form the subject of the present Paper.

It will be remarked that the plan, as described by the general terms of "suspension bridge with stiffened roadway," appears at first sight to be merely an old and widely-adopted construction; but in reality it is quite different. It adopts the stiffening principle as an integral part of the plan of the bridge, not merely for giving a degree of partial steadiness, but as at all times modifying the form of the entire bridge, and so combining the supporting powers of all parts of the bridge, for any one position of the load, that no partial load will effect a sensible partial disturbance. This arises, not from the use of suspending chains only (which, alone, would be liable to great disturbance of form), nor from the use of the road-stiffening only (which, alone, would be liable to great disturbance of form), but from a very peculiar effect of the combination of the two (which almost annihilates both disturbances of form). This principle appears to have been hitherto entirely overlooked; the explanation of it will form an important portion of the present Paper.

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[1] The discussion upon this and the preceding Paper occupied portions of three evenings, but an abstract of the whole is given consecutively.

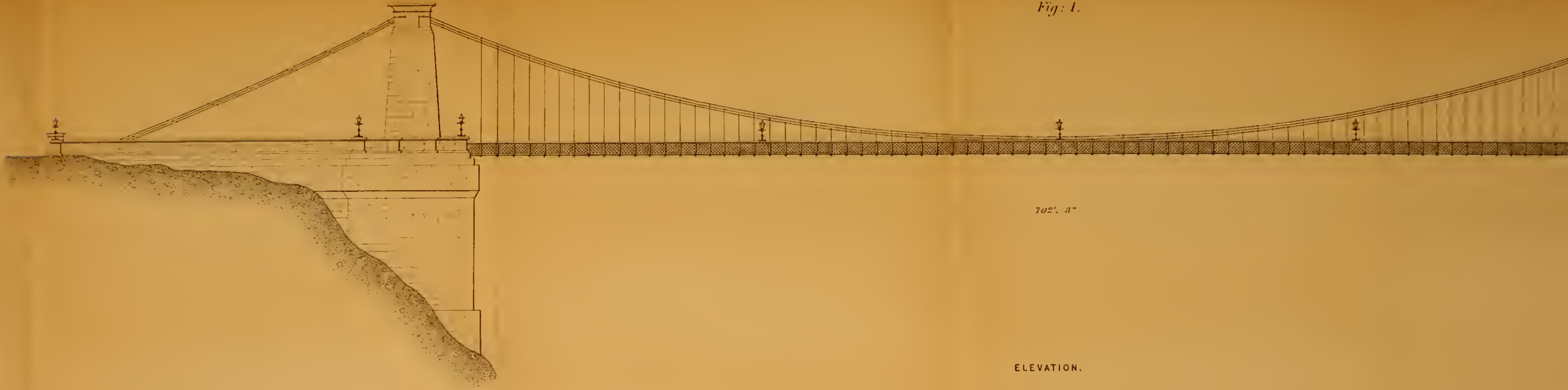
Suppose that a plain-jointed or flexible suspension-chain supports a stiffened roadway, by plain vertical suspension-rods (always in tension, and adjusted to bear as nearly as possible equal tensions through the whole series of rods). The stiffening of the roadway, in bridges of excessively large dimensions, may be a continuous tube; but, in bridges of the largest dimensions likely to be erected for many years, may be a continuous stiffened parapet, with sufficiently deep webs, and with strong tables above and below, adapted to bear equal strains of thrust and tension through all parts. Now suppose that a heavy load is placed on the centre of the roadway. The direct effect of the weight is, to depress the middle portion of the roadway, and, with it, to depress the middle portion of the chain. But the depression of the middle portion of the chain is necessarily accompanied with a rise of the flank portions of the chain; and this rise will raise the flank portions of the roadway. The roadway, therefore, will be bent into a serpentine form, consisting of two elevations and one intermediate depression; the length of the depressed part being not very different from $\frac{4}{10}$ths of the whole length, and the length of each of the raised parts being not very different from $\frac{3}{10}$ths of the whole length. And all these three curvatures (which, it will be remarked, are sharp curvatures) are produced by the action of one weight; a portion of its pressure acting directly to produce the central depression; and other portions of the pressure acting indirectly through the media of the central suspending rods, the tension of the chain, and the flank suspending rods, to produce the flank elevations. Now, on comparing these various portions of the central weight with a single weight which would produce the same depression upon the centre of the roadway, supposed to be wholly unconnected with chains, it will be seen that the portion of the central weight which depresses the central section of the roadway must (on account of the sharpness of the curvature in the short portion) greatly exceed the single weight; that each of the portions which raises the flank section of the roadway must greatly exceed the single weight; and, therefore, that the entire central weight, in the case of a roadway connected with a suspension-chain, must exceed, in a very large proportion, the single weight which would produce an equal depression on the same roadway unconnected with a suspension-chain. Or it may be stated thus: if the same weight be placed on the roadway, in one case connected with a suspension-chain, in the other case unconnected with a suspension-chain, the depression of the roadway in the latter case will exceed, in a very large proportion, that in the former case.

In reasoning roughly from these principles, it appeared probable to the Author, that the connection of a roadway with a suspension-chain would reduce the deflection produced by an accidental load to something like $\frac{1}{20}$th part of its former value. This, it will be remarked, is inferred entirely from the effects of the geometrical connection produced by the use of a chain, without taking into account the effect of the weight of the chain, which also tends to reduce the deflection produced by an accidental weight. Combining these considerations, it appeared probable to the Author, that the effect of connecting a roadway with a suspension-chain would be, to diminish the deflection produced by an accidental load to $\frac{1}{25}$th, or $\frac{1}{30}$th of its former value.

In order to verify this reasoning, the following experiments were tried. A suspension-chain was provided, somewhat exceeding 12 feet in length, weighing 18½ lbs., attached at its ends to two pins at the distance of 11 feet 11 inches; the central dip of the chain being about 1 foot 11 inches. A spline of wood exceeding 12 feet in length (intended to represent the stiffened roadway) was lodged at its ends on pins, vertically below the attachment-pins of the suspension-chain. The upper surface of the spline was furnished with fifteen screw-eyes, at nearly equal intervals, and these were connected with the suspension-chain by fifteen suspending-wires, adjusted to support the spline in a horizontal position, and to be all (as nearly as was practicable) in bearing at the same time. The experiments were four in number; Nos. 1 and 2 being made with a thick spline, and with loads of 10 lbs. and 20 lbs. respectively; and Nos. 3 and 4 being made with a thinner spline, with the same loads of 10 lbs. and 20 lbs. respectively. Before making the bridge experiments, some introductory experiments were made to determine the deflection of the same spline, produced by the same weights acting centrally, when the spline was supported on pins at different distances.

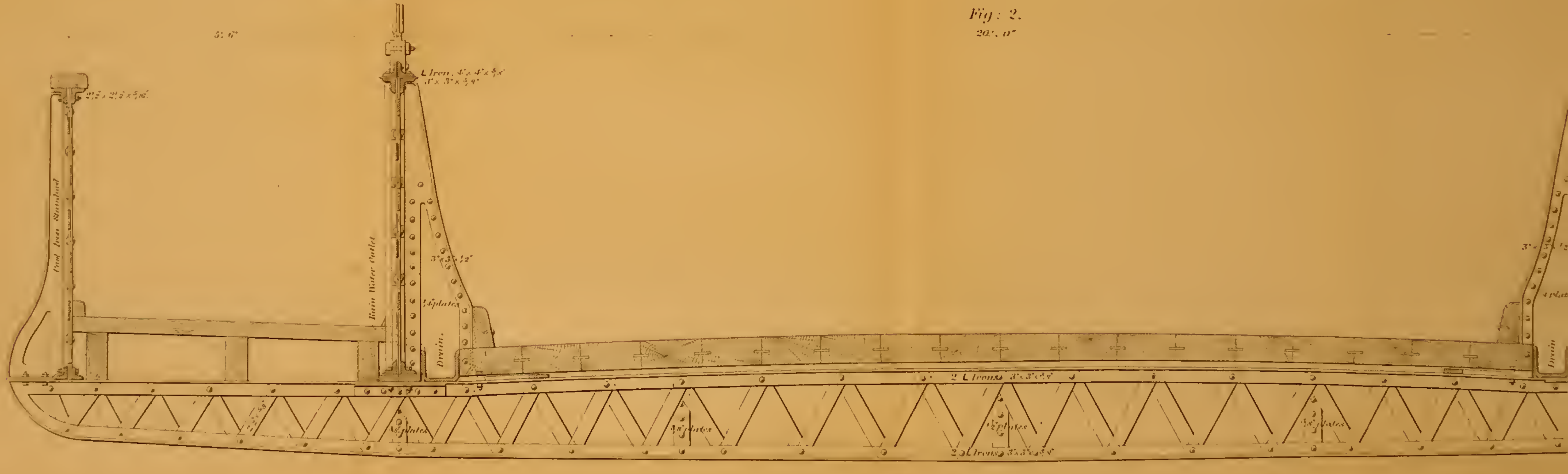
In the following experiments, the unit of vertical measure or of deflection of the chain or spline is the $\frac{1}{32}$ part of an inch.

Fig: 1.



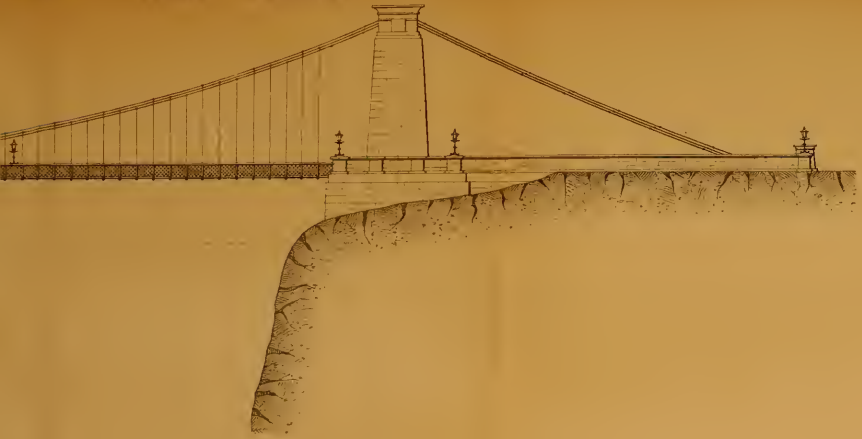
ELEVATION.

Fig: 2.



CROSS SECTION.

CLIFTON BRIDGE.



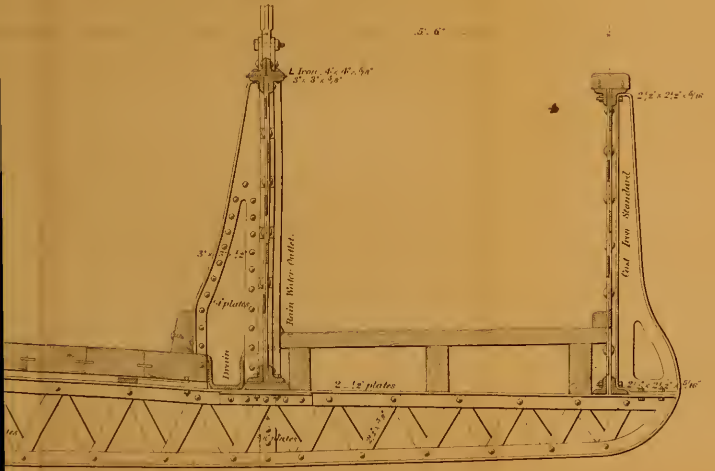
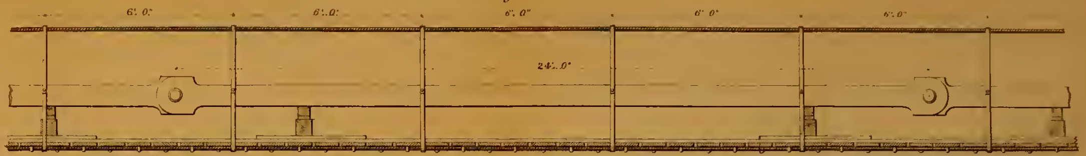
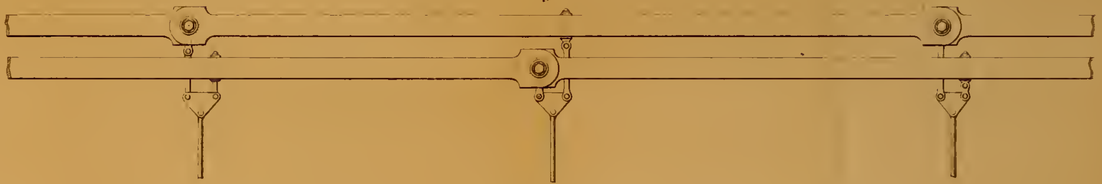


Fig: 3.



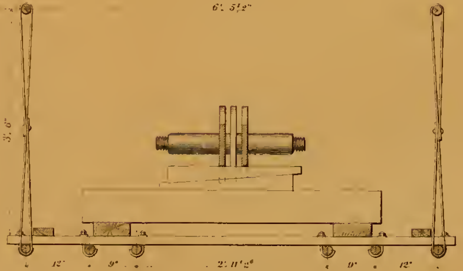
TEMPORARY WIRE ROPE PLATFORM.
ELEVATION.

Fig: 5

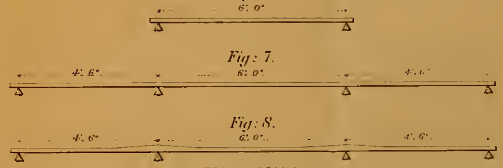


HUNGERFORD LINK.

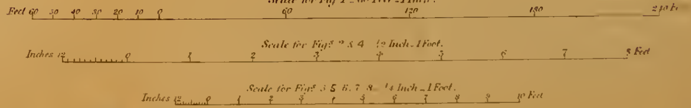
Fig: 4.



TEMPORARY WIRE ROPE PLATFORM.
CROSS SECTION



TRIAL BEAMS.


Scale for Fig 1 _ 60 Feet _ 1 Inch.
Feet 60 50 40 30 20 10 0 60 120 180 240 Feet
Scale for Figs 2 & 4 _ 1/2 Inch _ 1 Foot.
Inches 12 0 1 2 3 4 5 6 7 8 Feet
Scale for Figs 3 5 6. 7 8 _ 1/4 Inch _ 1 Foot.
Inches 12 0 1 2 3 4 5 6 7 8 9 10 Feet

EXPERIMENTS I. AND II. THE THICKER SPLINE USED.

Introductory Observations.

Deflection of the Spline produced by the Weight, when the Spline is supported on Pins at different intervals, and the Weight is placed Midway between the Pins.

Numbers.	Interval of Supporting Pins.		Deflection Produced by 10 lbs.	Deflection Produced by 20 lbs.
	ft.	in.		
1 and 2	3	0	1	2
3 and 4	3	6	1·6	3·2
5 and 6	4	0	2	4
7 and 8	4	6	5	10
9 and 10	5	0	7	14

Observations on the Bridge.

Numbers.			With 10 lbs. Weight.	With 20 lbs. Weight.
11 and 12	Chain entirely separated from spline, and weight hung on the middle of the chain.	Depression of centre of chain	78	93
13 and 14		Rise of chain at ¼ length. .	42	64
15 and 16	Chain separated as above and weight hung on the chain at ¼ length.	Depression of weight . . .	108	140
17 and 18		Rise of middle of chain . .	62	109
19 and 20		Rise at the other ¼ length .	100	160
21 and 22	Chain separated as above, weight hung on centre of spline.	Depression of centre of spline	44	82
23 and 24	Chain connected with spline by central suspending wire only, weight hung on centre of spline.	Depression of centre of spline	31	54
25 and 26	Chain connected with spline by all the fifteen suspending wires, weight hung on centre of spline.	Depression of centre of spline	2	5
27 and 28		Rise at ¼ length	1	1
29 and 30		Distance, from bearing pins, of point where the rise begins.	ft. in. 2 2	ft. in. 2 6

(It is probable that the last distances are too small.)

Numbers.			With 10 lbs. Weight.	With 20 lbs. Weight.
31 and 32	Chain connected with spline by all the fifteen suspending wires, weight hung on spline at ¼ length.	Depression of weight	4	8
33 and 34		Rise of centre of spline . . .	1	1
35 and 36		Rise of spline at the other ¼ length	2	4

EXPERIMENTS III. AND IV. THE THINNER SPLINE USED.

Introductory Observations as before.

Numbers.	Interval of supporting Pins.		Deflection Produced by 10 lbs.	Deflection Produced by 20 lbs.
	ft.	in.		
37 and 38	3	0	2·5	5·5
39 and 40	3	6	4	8
41 and 42	4	0	5	10
43 and 44	4	6	8	16
45 and 46	5	0	10·5	21

Observations on the Bridge (there is no need to repeat the observations from No. 11 to 20).

Numbers.			With 10 lbs. Weight.	With 20 lbs. Weight.
47 and 48	Chain entirely separated from spline, weight hung on centre of spline.	Depression of centre of spline	138	240
49 and 50	Chain connected with spline by central suspending wire only, weight hung on centre of spline.	Depression of centre of spline	40	60
51 and 52 53 and 54 55 and 56	Chain connected with spline by all the fifteen suspending wires, weight hung on centre of spline.	Depression of centre of spline	6	10
		Rise at ¼ length	1	2
		Distance, from bearing pins, of point where the rise begins.	ft. in. 2 11½	ft. in. 2 11½
57 and 58 59 and 60 61 and 62	Chain connected with spline by all the fifteen suspending wires, weight hung on spline at ¼ length.	Depression of weight . . .	9	17
		Rise of centre of spline . .	1	1
		Rise of spline at the other ¼ length	3	6

It appears to the Author that these experiments entirely support the theoretical views by which he was led to them. Their practical import will be best understood by extracting only those which relate to the central depression.

Drop of the Weght when attached to the centre, under different circumstances.

	——	I.	II.	III.	IV.
11 & 12	When the weight is carried by the chain, unconnected with the spline	78	93	78	93
21, 22, 47, 48	When the weight is carried by the spline, unconnected with the chain	44	82	138	240
23, 24, 49, 50	When the weight is carried by the spline, connected with the chain by the central suspending wire only	31	54	40	60
25, 26, 51, 52	When the weight is carried by the spline, connected with the chain by the fifteen suspending wires	2	5	6	10

Each number of the third line ought, unless the weight is great, to bear this relation to the two numbers above it, that the reciprocal of the third number is equal to the sum of the reciprocals of the two numbers above it; and this relation holds nearly enough, especially for the weight of 10 lbs. The weight of 20 lbs. appears to be too great, in proportion to the weight of the chain. The numbers of the fourth line show the astonishing effect of the connection of the chain and spline through the whole length of the bridge, as was predicted from theory.

The observations 27, 28, 53, 54, show the accompanying rise at $\frac{1}{4}$ length, which was predicted from theory.

The comparison of the deflections in 25, 26, 51, 52, with those in 5, 6, 41, 42, shows that the central depression in the composite bridge is equal to that of a tubular or girder bridge of the same section as that in the composite bridge but of one-third of the length, or less.

It appears to the Author that a construction founded on these principles would be the best that has been proposed for crossing very wide spaces. It presents none of the inconveniences attending any attempt at bracing, such as the difficulty of introducing thrust-rods of 100 feet or more in length, or the shock of back-lashing if the rods are adapted by elongated holes to sustain tension but to yield to thrust. In the construction here proposed, every suspension-rod receives a slight increase of tension from the presence of a moving load on any part of the bridge, the tension beginning gradually when the load enters on the bridge, increasing gradually till the load is abreast of the rod (when it is still small), and decreasing gradually till the load quits the bridge. The construction adapts itself to changes of temperature.

It appears that the form of the bridge is disturbed in a somewhat greater degree by a load at $\frac{1}{4}$ length than by one at the centre.

It appears also that there is a slight tendency to throw the distant end of the girder upwards, the end sinking gradually as the weight approaches it. The inconvenience, if there is any, can be met by a small dead load on the end.

For planning the tube or girder of such a bridge, the rule founded on known constructions is very simple. The Engineer will decide mentally on the amount of deflection which he will tolerate in the bridge as the effect of a given load. He will then, by the rules of engineering, compute the section of the tube or girder which will permit that deflection in a bridge of $\frac{1}{3}$ of the length in question, or less; and this will be the section to be used for his long bridge. Thus for a span of 600 feet it would be sufficient to use the section of a girder whose length is only about 180 feet.

The same consideration gives the limit to the width which can be safely crossed by this construction. Suppose it to be accepted that a simple tubular bridge can be trusted (not as regards absolute safety, but as regards deflection by loads) to the extent of 500 feet. Then the composite construction can be trusted to the extent of 1,500 or 1,600 feet, in so far as depends on the stiffness of the tube and the peculiar effect of its support by chains.

This limit is not reduced by consideration of the strain which the chains can bear. The Author believes that the tensile power of an iron bar is able to carry safely a length of about 2 miles of the same bar. If the whole compound weight of chain, roadway, and load is supposed to be triple the weight of the chain alone, then the length of the united mass which the chain can carry will be about 3,500 feet. Now, in a catenary of any kind, the tension strain at the centre (from which that at the flanks does not much differ) is the weight of a length of the united mass equal to the radius of curvature, which usually does not much exceed the span of the bridge, or (as supposed above) 1,500 or 1,600 feet. It appears therefore that, on the side of the chains, everything is safe.

It would be proper to ascertain by experiment (duly interpreted) the deflection of the proposed bridge at different points which will be produced by expected loads, and to build the tube or girder on shore, with a curved base, whose elevation, at every point, above the line joining the extremities, is equal to the deflection at that point which is to be counteracted. But it is not necessary to build the girder in one piece. It may be in any number of pieces of convenient size, each of which is firmly connected in itself, and each of which has the rivet-holes accurately prepared to fit to the adjoining pieces. It is presumed that there is no special difficulty in mounting the chains with their suspending-rods, the lower ends of those suspending-rods being accurately adjusted to lengths corresponding to the slightly-curved form (above mentioned) of the girder. Then the successive pieces of the girder can be pushed on or raised up, and, in order to make the rivet-joints correspond properly, temporary weights must be applied in different parts of the flexible bridge to disturb its form. In this way, it is believed, the lines of prepared rivet-holes can be made to correspond with the utmost delicacy.

Mr. W. H. Barlow said, since the Paper was written—and, in fact, since the last meeting of the Clifton Bridge Company and the issue of their Report—they had had the misfortune to lose by death the late Captain Huish (Assoc. Inst. C.E.), who was the Chairman of that Company. He considered the undertaking was so much indebted to that gentleman's efforts, in conjunction with those of Captain Claxton, the Secretary of the Company, that he could not allow the opportunity to pass without expressing the great regret he felt, which he had no doubt was shared in by those present, at the loss of so valuable a member. The only other remark he wished to make was that, standing forward as he did as the Author of the Paper, it might be considered that he had personally taken a more active part in the construction of this work than his colleague, Mr. Hawkshaw. He, however, wished to state that they were in every respect strictly the joint Engineers of the work: whatever was right was due to both; and in whatever was wrong, both must be condemned.

Captain Claxton, R.N., as late Secretary to the Clifton Bridge Company, stated that it was owing to the Engineers, who took up the shares so liberally in London, incited thereto by Messrs. Hawkshaw and Barlow, who were no doubt desirous of perpetuating a great work of art, that the Clifton and Bristol people were induced to follow their example, though they had previously spent 50,000*l.* to their loss. With respect to the span of the bridge, and of others of large span alluded to, he would say, from his own knowledge, that the original design of Mr. Brunel was to put the left-hand pier 260 feet further back on the rock, which would have made the total span 960 feet. But Directors did not always let the Engineer have his own way; and the Directors of the Bridge Company did not let Mr. Brunel have his way in this matter—hence that enormous abutment. He repeated, but for the liberal assistance of the leading Engineers of the country, the bridge would not have been built as it had been.

Mr. G. W. Hemans did not wish to make any criticism on this beautiful bridge, which was the largest suspension bridge in this country, except with regard to the test-weight, which was stated to be 70 lbs. to the square foot. It struck him that was rather low to assume as a possible weight that might come upon the bridge. For his own part, when he had bridges to build for roadways, he calculated a test-weight of 100 lbs. to the square foot, as he thought, in a very dense crowd, there might be one man in a square foot, or nine men in a square yard, and therefore he thought 70 lbs. per square foot as a test-weight was rather low, although he believed that weight had been taken in the case of the new Westminster Bridge. With regard to the oscillation of this bridge, he would ask Mr. Barlow whether it had not occurred to him to stiffen this structure, as was done in the case of the Niagara Suspension Bridge—by the use of diagonal chains extending from near the centre of the platform of the bridge down to the foot of the abutments. These chains need not necessarily be in the vertical plane of the bridge, but, being carried at a considerable angle laterally, they gave a tie to the bridge both in a vertical and in a horizontal direction. There could be no doubt the effects of heavy gales of wind on bridges of this sort were very great indeed, and, therefore, that mode of stiffening the structure might be worth consideration, particularly when further extension of the spans of such bridges was contemplated.

Mr. R. P. Brereton recollected the original designs for this bridge, and the subsequent attempts made by Mr. Brunel to get the work resuscitated, after it was stopped for want of funds. It was intended to have under-chains extending from the abutments below to the under side of the bridge, for the purpose of giving additional vertical and lateral stiffness. Thirty years ago the means of getting longitudinal stiffness by the use of the wrought-iron contrivances, which were now adopted, was not known, and it was thought at that time that the under-chains were more indispensable than they had subsequently been found to be; but they certainly formed part of the original design for this bridge. There were also suspending-rods along the land chains, the object of which was to prevent the deflection in the middle span when loaded, which afterwards occurred; but they had been abandoned, probably more from motives of economy than anything else. He gathered from the Paper that there was an inclination in the beds of the saddles of 1 in 20. If that were so, he did not know the reason for it. The original intention was that the base of the saddle should be level, as it was in Hungerford Bridge, admitting of free movement of the spans when loaded, and recovering when the load was taken off, so that the resultant forces should be always vertical. Mr. Barlow, no doubt, had a reason for adopting an inclination in the beds of the saddles.

Mr. Fowler, President, asked whether Mr. Brereton could give any reason for Mr. Brunel placing one end of the platform of the bridge 3 feet lower than the other.

Mr. Brereton replied that the bridge would have looked out of level, owing to the difference in the height of the two piers; and the alteration of strain, by the lowering of one end 3 feet, was quite inconsiderable.

The Astronomer Royal remarked that, it having been intimated that a few words from him on this subject would be acceptable, all he could address himself to would be in a general point of view. It would be understood that a person so unpractical as himself was not competent to make remarks on special points, such as the measure of testing weight, and questions of that kind. He did not like the old plan of Telford's of taking successively the bearings upon different chains, and he exhorted Engineers by all means to avoid it. The old Hungerford Bridge was bad in principle in this respect, that the pressure was placed on the middle of the link; but, in reference to the arrangement for throwing every pressure upon the two chains, it was preferable to the plan he had alluded to. He considered this principle important for giving to suspension bridges the whole intended strength of the chains in supporting those parts that had to be supported.

There was only one other point: a large portion of the latter part of the Paper was devoted to a consideration of the combined principle of supporting a bridge and making it stiff. A good many years ago his opinion was asked by the late Mr. Stephenson on points connected with the construction of the Britannia Bridge, and he then gave his opinion that the two principles ought to be combined in that construction. A little time ago he put the matter more into shape, and he had made a rough model of what he proposed. His views upon it were fully explained in the Paper he had submitted, "On the Use of the Suspension Bridge with Stiffened Roadway."[1]

Mr. G. K. RADFORD mentioned that a suspension bridge had recently been erected by Mr. Roebling, the Engineer of the Niagara Bridge, over the river Ohio, at Cincinnati, U. S., consisting of three spans, the centre one being 1,000 feet, and side-openings about 250 feet each. There were two cables, 12½ inches in diameter, made of iron wires untwisted, bound up with wire, and served with hemp yarn, painted for protection from the weather. The deflection of the cables was 88 feet, and they were about 60 feet apart at the saddles on the piers, and 36 feet at the centre of the bridge, so that the suspending ropes had an inclination inwards, which assisted in giving lateral stiffness. The suspending ropes were ordinary wire ropes, 5 feet apart, at each roadway beam, to which they were secured by cast-iron blocks, and wrought-iron screw bolts of about 3 feet long. The block was cast with a bevelled hole in the centre, and a hole for a screw bolt at either side. The lower end of the rope was passed through the bevelled hole, the strands opened, and nails driven in, until the rope was swelled out to fill the hole. This made a very secure and simple connection. The upper end of the rope was secured to the cable by iron clamps. In addition to the vertical suspending ropes, there were others radiating from the saddles to the roadway beams, for a distance of about one-quarter of the span. The roadway, 36 feet wide, consisted of a carriage-way and two footways. The roadway beams were of rolled iron, 12 inches deep, with 5-inch flanges, weighing 20 lbs. per foot, rolled 19 feet long, and connected by flitch plates.

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[1] *Vide* ante, p. 258.

The stiffening girders, 10 feet deep, were placed between the carriageway and footways, and wholly above the roadway. They consisted of top and bottom members, formed of channel irons placed vertically back to back, with vertical struts, and diagonal bracings. The bottom member rested upon the roadway beams, and was secured to them by a strap under the beam. The roadway was formed of planking, with rails for a street railway, and had a considerable longitudinal camber. The under-side was 100 feet above low water, and 60 feet above high, or flood water. The piers were of stone.

Mr. Henry Law mentioned that under-chains, as a means of preventing oscillation from the effects of high winds, were adopted by the late Sir Isambard Brunel in two suspension bridges erected by him in the Isle of Bourbon, in 1823.

Mr. C. H. Gregory, V.P., could not help thinking the plan adopted by Mr. Hawkshaw and Mr. Barlow for stiffening a suspension bridge by girders was a better way of preventing undulations from wind and other causes than the under-chains which had been alluded to, and that opinion was confirmed by the small amount of vertical motion which was reported to take place in the Clifton Bridge during gales. He remembered—many years ago—seeing the Menai Bridge, when acted upon by only an ordinarily strong breeze, and there was a much larger amount of motion than had been reported and measured in the case of the Clifton Bridge. Another advantage in that mode of stiffening was that it equalized the weights, so as to counteract the evil alluded to by the Astronomer Royal, as arising from the suspending-rods being attached to points in the same sets of suspension chains which were not opposite to one another, while it avoided the alternative evil of attaching the suspending-rods in such a way as to throw a cross strain on the links of the suspension chains. In the Niagara Bridge the stiffening of the roadway was, in fact, effected by lattice girders; and although there were under-chains, he believed that they were not practically of much use in that structure.

Mr. R. Richardson apprehended that those who had spoken of the under-chains assumed the bridge to be imperfect. There was no occasion for under-chains in this case. A bridge, to be a perfect machine, ought not to need under-chains; and it only showed proper economy, which was the true wisdom of Engineers, in avoiding the necessity for these under-chains. With regard to the exception that had been taken to the testing weight of 70 lbs. to the square foot, he might state that 80 lbs. to the square foot was the French Government test for railway bridges. In this country 105 lbs. was the standard test. Mr. Barlow had selected 70 lbs. as being quite sufficient, and in all probability there would never be a traffic over this bridge such as would give a pressure of 60 lbs., and therefore there was no fear of undue deflection on that account. The lateral deflection was scarcely worth observation. But there was one matter in reference to this bridge which he looked upon as serious. He agreed that steel was a material applicable to an immense span; but he begged Engineers to consider seriously the effect of mixing up iron and steel together. If there were metals of different qualities in a structure, there were different elements of flexibility. In the construction of the Lambeth Bridge there was more flexibility, owing to the superior metal of the wire ropes, than there was in the other metal; and, but for the trussing, this mixing of ordinary with better metal would produce an unequal vibration. In making a bridge, he would advise it to be all of steel or all of iron, bearing a certain test. Mr. Barlow allowed 10 tons to the square inch of iron, whereas steel would take 35 tons to the square inch.

Mr. de Bergue considered the under-chains not only unnecessary, but objectionable, as enhancing the chance of casualties from the contraction of the metal in severe weather, because the bridge would be drawn upwards by the contraction of its own chains, and the under-chains would, from the same cause, draw it in a downward direction; and these under-chains would be liable to break, as, he believed, had been the case at the Niagara Bridge.

Mr. Cochrane would confine his remarks to matters connected with the erection of the Clifton Bridge. During the time the work was proceeding, when the platform was considerably advanced, and before the chains were put up, some heavy gales of wind were experienced. At that time a large rope had been attached from the centre to the rock on each side, to provide against the undulations produced by the gales. During a heavy gale the rope broke, and the centre of the bridge rose considerably. This taught him a lesson, and he afterwards put in more guy-chains, halfway between the centre and the pier, on each side. When the rope broke the first time, the centre rose above the level more than 70 feet, and then came down again with a force that was anything but pleasant. The wire ropes over the temporary saddles were considerably abraded at the rubbing surfaces, and it was feared they would have to be renewed, but, on examination, the mischief was found not to be so serious as was expected. When the additional guy-ropes were introduced, to keep the temporary staging down in its proper position, they were found to answer very well, though in a gale of wind there was considerable vertical motion. It was stated in the Paper that the crane used was constructed to lift one cross girder, and a portion of each longitudinal girder attached at the same time. The process was to take one piece of longitudinal girder, in length about 16 feet, which was suspended on the crane while it was being fixed, and the longitudinal girders were carried on about 16 feet in advance of the cross girders. He would take this opportunity of returning his thanks to Mr. Airey, to whom was due, in a great measure, the ingenious contrivance adopted for the erection of this structure. The work was carried out without any accident whatever, and with the loss of only two lives, occasioned by men falling either in fits, or from causes beyond control. The system of temporary staging was mainly of Mr. Phillips' devising, but to Mr. Airey was due the credit of scheming the general arrangements for carrying out the work; to whom, as mark of appreciation of his services, he might add that the Directors of the Company had presented the sum of £100 on the completion of the work.

Mr. J. A. Longridge would be glad to hear from Mr. Barlow the motives which led to this bridge being built on a camber of something like 2 feet, as he confessed he did not at present see what object was to be gained by it.

Mr. Vignoles said he had had some experience in the construction of suspension bridges, though he was afraid he must count the time since he was engaged in such works by decades rather than by single years; and that reminded him that he had neglected his duty to the Institution in not having long since contributed a description of what he had done himself. Procrastination, in this instance, was truly "the thief of time;" but it might not be too late to state his views of the principles on which he considered suspension bridges should be constructed. In the first place, with respect to the cross girders, it had been stated that a great deal of the strength of the platform was due to the stiffness given to them. He thought that principle had not been carried far enough. He recollected when the first serious accident occurred to the suspension bridge of Telford, over the Menai Straits, the late Mr. Rendel recommended that additional depth should be given to the cross girders of the platform. Mr. Vignoles had taken advantage of that suggestion in the constructions on which he had been engaged. He not only brought the two longitudinal beams, connecting the cross girders above the platform, as high as possible, making them act as parapets between the carriage-way and the footpaths on each side, but he had carried these and the cross girders to a still greater depth below, and the result was a stiffness so great that, in a bridge which he constructed for the Emperor of Russia, artillery could be driven over it at full gallop without producing the least undulation, and the heaviest gales had had no appreciable effect upon it whatever—certainly none of the lateral effect spoken of. He believed that remarkable amount of freedom from vertical movement, or undulation of the platform, was in part owing to breaking joint, so to speak, with the suspension-rods. When he decided to adopt a deeper section for the cross girders, it was also settled that the suspension-rods should not be opposite to each other, but should be intermediate to one another, break joint, as he might say. The rods were placed at intervals of 6 feet, the length of the links being 12 feet; at every alternate 6 feet there was a suspension-rod breaking joint, and thus undulating motion was prevented, inasmuch as the rods were not acted upon at the same moment. Therefore, he contended, if the cross girders of the Clifton Bridge had been brought down to the depth of 8 or 9 feet, or more, below the platform in the same manner, great increase of strength would have been given. He did not at all advance this as an idea of his own, but it was following out the principle enunciated by Mr. Rendel, of giving greater depth to the elements of the platform, and imparting the greatest strength to it. The bridge alluded to had five middle openings of 440 feet, and two side openings of about 220 feet. There was another point, also, with respect to suspension bridges, which he thought had not been sufficiently attended to in practice, and he would ask the Astronomer Royal, as a mathematician, to confirm the accuracy of what he should enunciate, viz., that the strains on each side of the top of the pier should be exactly the same; and, consequently, that they should, if possible, go off from the pier on each side at the same angle. The equalization of the strains could be calculated precisely, and it would generally be found, that the point at which the back chain should touch the abutments would be below the platform level, and not above it. He thought in the case of the Clifton Bridge unequal strains would be found to obtain both practically and theoretically, supposing the angles at which the chain left the top of the pier were not exactly the same on each side. If the angles were not equal, the strains could be made equal by altering the form of the back-chain. He had not the formula at hand for this calculation, but the general principle was, that the strains on the top of the pier should be made equal. He was aware that a number of failures in the French wire bridges had taken place, as he believed, from the want of proper attention to this principle. The weight of the platform he constructed in the bridge he had alluded to was not more than 15 per cent. heavier than that of the Menai Bridge; and if, with the addition of 15 per cent. to the weight, he got three times the strength, there could be no doubt as to the great advantages obtained. Most of the failures had arisen from giving too little depth to the platform; and from inequality of strains at the points of suspension.

Mr. Brereton remarked, that Mr. Vignoles started with the statement that the angles of the chains ought to be equal on each side of the pier. Now, the great object was that the resultant strains should be vertical on the pier. This was particularly required in the case of the light piers of the Hungerford Bridge, as the chains of the side spans were at steeper angles than the main opening. There was no fixed connection between the chains and the tops of the piers, but they rested on horizontal rollers, which enabled the saddles to travel on either side as the angles varied from the irregular loading of the different spans. It was not possible on horizontal rollers to get anything but a vertical strain downwards. If equal angles were carried out on both sides, there must be a repetition somewhere of the varying angle, or the chains could not be got into the ground to the attachment of the anchorage. A back tie sufficient to resist the strains of the main span of the bridge was necessary. It was quite unimportant what was done on the land side. So long as no harm happened to the piers, the object was to get to the anchorage point as soon as practicable. French Engineers had believed that they could almost carry down the back chains vertically, and had overdone it. In the case of bridges over the Loire, the wire ropes were brought over the tops of the piers and down under the foundations, so that an irregular strain was brought upon the piers; but Mr. Vignoles did not give credit to the saddles, which enabled the chains to travel without variation in the resultant weight upon the pier.

Mr. VIGNOLES did not say the back chains could be carried anywhere, but calculations could be made upon a fixed principle whereabouts the back chains should touch the abutment.

Mr. J. A. LONGRIDGE thought the Astronomer Royal would agree that, if the angle between the back chain and the other were bisected, and if the line bisecting this angle fell within the base of the supporting pier, the bridge would be stable, whether the bisecting line were vertical or not.

Mr. CALLCOTT REILLY remarked, if the suspension chain was passed over a fixed point or roller, which did not slide on the top of the pier, then if the angles with the horizon, on each side, made by the tangents to the curve of the chains, were equal, the resultant was vertical. If the angles were not equal, the resultant would pass in the direction which bisected the angle. There must be great friction in any such case; and that, together with the horizontal component of the resultant, would produce a bending moment upon the tower, tending to overturn it, which must be provided against by proper means. There was an example of this in the Lambeth Bridge, where the two chains made different angles, and this was met by giving sufficient transverse strength and stiffness, in the horizontal direction, to the wrought-iron towers, which acted as upright cantilevers in resisting the bending moment. In the more usual case, where the ends of the chains were made fast to a moveable truck, resting on rollers on a horizontal bed on the top of the pier, or tower, then the resultant would be vertical, whatever the relations between the angles of the chains, the condition being that the horizontal components of the two tensions must be equal.

Mr. RICHARDSON said, it was not necessary that the angles of the chains, with the summits of the piers, should be the same. The arrangement adopted in the Lambeth Bridge had been dictated by motives of economy.

The ASTRONOMER ROYAL was of opinion that the true principle had been correctly stated by Mr. Reilly. This would be better seen if two cases were stated—one in which the chain was fixed to the top of the pier, or in connection with it to such an extent that it amounted to the same; the other, in which the chain moved freely over the pier. If there was no fear of expansion or contraction, the chains might stand at any different angles whatever; but if it was in the nature of a chain running over the top of the pier, then the resultant force would bisect the angles formed by the two chains, and Engineers must be guided by their judgment as to how far they would allow the direction of that force to depart from the vertical. If, however, the base of the pier could be spread out, so that the direction of the resultant force fell within that base, then it might do. He brought this forward as a theoretical point only. But it was no idle thing. He remembered the erection of the first wire bridge over the Seine in the year 1828 or 1829, and it tumbled down from too great steepness in the land-side chains. With regard to the lower brace rods, to which allusion had been made, there was one way of considering their effect to which attention had not been directed. A bridge was either stiff or not. If it was quite perfect, of course it did not want any rods; but if it was not stiff, at one time the rods would be relaxed, and at another they would come up with a chink or violent blow. That was an element of danger to which no allusion had been made by those who had spoken on the subject.

Mr. LE FEUVRE said, reference had been made in the Paper to the suspension bridge of which he and his partner, Mr. Ordish, were the Engineers. Mr. Barlow had stated that that principle of construction was not, in his opinion, applicable to multiple spans. It was a question whether the principle of suspension was applicable to multiple spans, in consequence of the enormous quantity of metal which was necessary when there were unequal loads, when one span might be loaded, and others not. It was then necessary to have sufficient metal in all the spans to allow for the loading of any particular span. The bridge to which he referred was particularly applicable to railway purposes. That was the original intention of the system; and he might say there was great economy in constructing a bridge of this character, in consequence of having a combination of curved and straight chains. This system was peculiarly applicable for bridges having one large central and two smaller side spans, the proportion of the larger span, being four times the width of the smaller spans.

Mr. HAWKSHAW would ask one question of the Astronomer Royal. He had stated that the section which would do for a girder of 180 feet span might be applied to a bridge of 600 feet span. He would be glad to know whether in that statement the Astronomer Royal had included the sectional area of the chain as well as the girder?

The ASTRONOMER ROYAL.—Not of the chain.

Mr. CALLCOTT REILLY said, the problem of economically designing a suspension bridge of several spans, adapted to carry a railway load, was one of great interest, and opened up a variety of questions. The two most important, perhaps, were:—1st. How to get a suitable minimum of deflection in any one span, considered by itself, under the action of the heavy travelling load; and next, how to get rid of the accumulated deflection, which in a multiple span bridge would occur in one span when fully loaded by the travelling load, while the other spans were unloaded. The first condition seemed to be solved in a simple and admirable way by the ingenious invention of Mr. Ordish, wherever there was a single span only, or one wide span combined with two short side spans, and that seemed well adapted for giving a small deflection under a heavy moving load upon the central span. But if it were attempted to apply that system to a series of large spans, then, as remarked by the Author of the Paper, great difficulties would present themselves. There would be an accumulated deflection caused by the shortening of the versed sine of the chains in the unloaded spans, and the consequent travelling towards the loaded span of the moveable saddles on the tops of the towers, producing an accumulated depression of the chains in that span. The only way in which that could be avoided in a bridge of that kind of construction, or in any kind of suspension bridge stiffened by trussing the chains, would seem to be to fasten the chains to the tops of the towers, and to secure them, so as to prevent their travelling longitudinally. That, however, raised a difficulty in the way of building lofty towers. If, for instance, towers 200 feet high were required (and it must be remembered that very wide spans were generally combined with great height), they must be built as upright cantilevers of that height, requiring great transverse strength to prevent the top yielding to the horizontal pull, due to the unbalanced load; and the difficulty of doing that might be imagined. Therefore he thought Mr. Ordish's system was only applicable to such cases as that of the Albert Bridge alluded to in the Paper. There was another plan projected and executed by Mr. Peter Barlow in a bridge of several spans at Lambeth. But if that system was carried out with accuracy, and with a view to get all the advantages of trussing between the chains and the platform, it seemed that the difficulties just alluded to must be encountered. Mr. P. Barlow had done that in some of the towers by making them of wrought iron of the proper strength and stiffness considered as upright cantilevers. In that bridge it was an easy matter, because the towers were neither lofty, nor the spans wide.

There was another plan which had been executed in a bridge in Germany, the principle of which was investigated in Mr. Latham's book,[1] which consisted of a 'hanging girder,' having two chains on each side of the bridge parallel to each other, and one below the other, with a considerable depth between them. The vertical space between the chains was filled in by a series of diagonal bracings, which converted the pair of chains into a curved triangular girder. In a multiple span bridge, the advantages of that plan could not be realized without encountering the difficulty relating to the towers which he had previously mentioned.

The effects of change of temperature, in all the preceding cases, would be a serious consideration. For instance, reverting to the second kind of construction he had mentioned, that of diagonal trussing between the chains and platform, that kind of bridge had been very accurately investigated in a work by a German mathematician, published a few months ago in English,[2] and it was shown that a change of temperature of 54° Fahr., compared with the temperature at which the bridge was built, would vary the stress upon all the different bars by as much, in some instances, as 40 per cent. Another circumstance connected with that system of trussing between the chains and platform was worthy of notice. If the system were carried out with the intention of making a structure in which the stress upon every bar, under all conditions of loading, should be capable of accurate valuation, the structure would not be a suspension bridge at all, although possessing, for certain situations, important advantages of its own. It would, in fact, require a bottom horizontal member quite as important as so-called chains, and nearly as heavy, and the chains themselves would no longer be chains at all, for nearly one half of their length would be subject to compression; in fact, the stresses, in both chains and bottom member, would vary from tension to compression, and the reverse, much in the same way as in a continuous beam of analogous form. It would be necessary, to the fulfilment of the conditions of such a bridge, that the ends of the chains and of the bottom member should be rigidly secured to the piers at the four points.

From the preceding reflections it would seem that, to adapt the principle of suspension to railway bridges of several large spans, it would be necessary to fall back upon the method of stiffening by means of suspended girders, some of the advantages of which had

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[1] *Vide* Latham on "Wrought-Iron Bridges," 1858, p. 263.

[2] *Vide* "Skeleton Structures," by Olaus Henrici, Ph. D. Atchley and Co., 1866.

been so well explained by the Astronomer Royal. Mr. Peter Barlow, it was well known, some ten or twelve years ago, made a series of important experiments on this system, from which he deduced very promising results. It had since been accurately investigated by Professor Rankine, in his book on "Applied Mechanics."[1] The subject was then investigated theoretically, and a series of exact results were deduced which he thought could not be impugned. Professor Rankine proved that the necessary strength of the stiffening girder would be $\frac{1}{7}$th part of that of an independent girder, of the same span as the bridge, suited to bear an uniform moving load of the same intensity.[2] The most unfavourable condition of loading would be when the moving load covered two-thirds of the span. Under that condition the greatest deflection of the loaded segment below its original level, if that were supposed horizontal, would be almost exactly one half of that of the same beam uniformly loaded by a load producing the same intensity of stress upon the booms of the girders if unsupported by the chains. The stiffening girders, although comparatively so light, would yet require great depth, and consequently a considerable quantity of lateral stiffening material, which, however, might be economically applied underneath the roadway, supposing there was sufficient headway for the purpose. The required depth might be calculated in this way for a span of, say 700 feet—about that of the Clifton Bridge:—Assuming the safe working deflection, under the most unfavourable condition of loading, to be $1\frac{1}{2}$ inch per 100 feet of span, or $\frac{1}{800}$th of the span, and the intensity of the stress upon the sections of the booms producing that deflection to be 5 tons per square inch, equal to about $\frac{1}{2000}$th part of the modulus of elasticity, then the depth of an independent girder of uniform section, to satisfy those conditions of deflection and stress, would be about

$$700 \times \frac{5}{24} \times \frac{800}{1} \times \frac{1}{2000} = \frac{700}{12} = 58 \text{ feet } 4 \text{ inches,}$$

or $\frac{1}{12}$th of the span.[3] The required depth of the stiffening girder would be one half of that, or $\frac{1}{24}$th part of the span, in order that its deflection and intensity of stress should be the same as those of the independent girder.

In order to realize these results a condition was necessary which he observed had not been fulfilled in the Clifton Bridge, that was, that the ends of the stiffening girders must be held down at the piers while allowed freedom for end-long movement. It was quite easy to fulfil that condition, and it had been done at the Chelsea Bridge and in other instances. The effect of temperature had not been calculated by Professor Rankine, but it had been considered by Mr. Latham in his book before alluded to. The general conclusion appeared to be, that for spans up to 400 or 500 feet there would be no economy in the use of the suspended girder over a simple girder bridge, but that an increasing margin would appear in spans beyond those dimensions in favour of the suspension bridge.

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[1] *Vide* "A Manual of Applied Mechanics," by W. J. M. Rankine. Griffin and Co., 1858, pp. 370—5; and 2nd edit. of same, 1860, p. 375.

[2] Moving load only is considered in making this comparison, as the entire fixed load, including the weight of the girders, is supported by the chains.

[3] *Vide* Rankine's "Applied Mechanics," Art. 302.

The Author of the Paper had invited discussion on the subject of the proper proportions of the pins and eyes for connecting together the links of the chain, and he had alluded to the experiments instituted by Sir C. Fox on the links he made under the direction of Mr. Vignoles for the bridge at Kief. But quite apart from the question experimented upon by Sir C. Fox, it would seem evident, from an *à priori* consideration of the subject, that bearing-surface to resist pressure was an element, to be calculated in settling the proportions of the pins, quite as important as was the cross-sectional area to resist shearing; generally, indeed, much more so, because the latter element was usually in excess. This question was first systematically treated in Mr. Latham's book before referred to. The bearing-surface of the pin, perpendicular to the line of action of the pressure, was to be found by multiplying the diameter of the pin by the thickness of the bar at the hole, being, in fact, the area of the projection of the semicylindrical surface of the hole upon the plane of its diameter, providing the pin completely filled the hole. If it was a slack fit, the bearing-surface would be the area of the projection upon its diameter of that segment of the cylinder which was practically in contact with the surface of the pin. If that was admitted to be the rational mode of estimating the area of bearing-surface, the next question was, what should be the maximum intensity of the pressure upon it. First, let the bridge be supposed perfectly rigid, and that there was no rotatory movement between the surface of the hole and the surface of the pin. In this case there seemed no valid reason for permitting a greater intensity of pressure upon the pin than of compression or tension upon the section of the bar itself. That was to say, the pressure upon the bearing-surface of the pin must in no case exceed the least limit of elasticity of compression of the materials of the pin or eye. But if the intensity of the pressure be taken as great as that limit, it would be necessary in estimating its amount, to consider the dynamical effect of the heavy moving load (supposing the bridge to be a railway bridge) travelling at express speed, and it could be shown that in such a case the instantaneous pressure upon the surface of the pin, produced by that part of the total load, was nearly double the pressure produced by a load of the same nominal intensity put on bit by bit.[1] If, as in any practical instance, the condition of perfect rigidity did not exist, then there would be a rubbing of the bearing surfaces of the pin and hole, which would produce wear, and ultimate slackening of the joint, if anything like the preceding limit of intensity of pressure was attained. It had been suggested that the best way of meeting that evil would be to case-harden both wearing surfaces.

Mr. Peter Barlow said, he had listened with attention to the Astronomer Royal's observations and experiments, the fact being that similar experiments were made by himself in the year 1857, when he read a paper on the subject before the British Association, and arrived at similar results.[2] However, the mere result of experiments of that kind he did not think sufficient to determine the best construction of a suspension bridge. Many practical points arose which could not be determined by experiments on a small scale. When he was called upon to construct the Lambeth Bridge he made a trip to the continent and to America, on purpose to examine bridges of a peculiar construction which existed there. He had seen the Niagara Bridge and also a very large bridge near Bordeaux, the Pont de Cubsac, which he believed was the largest suspension bridge in the world. One of the results he arrived at was this, that the platform of every suspension bridge should be made entirely of wrought iron. The longitudinal and cross beams were now made of wrought iron, and the additional cost required to make the platform perfectly of wrought iron would be less than the cost of wood. Then the means of vertical rigidity, and a horizontal beam of great strength would be obtained. In the Lambeth Bridge, in the heaviest gale, there was no more motion than on a stone bridge; and this arose simply from the platform being a horizontal beam of great strength. That was not the only advantage of having it entirely of iron. The horizontal beam of iron, when combined with the supension-chain, might be made a means of vertical stiffness, as well as of horizontal stiffness. It was also of importance in another respect. The Pont de Cubsac consisted of five openings, and the point had been raised that, in a bridge of that kind, with a number of arches, if one arch was loaded the towers would be pulled over, and therefore, though it was easy to make a bridge of one span, a difficulty arose in the case of a number of spans. The difficulty was met at the Pont de Cubsac by carrying a cable from the top of one tower down to the foot of the other, and in that way tying the towers together. But these long chains, being unsupported, easily vibrated. During his visit to this bridge it was put into vibration from end to end, 1,800 feet, particularly the long cables from tower to tower, merely by a man and boy dancing upon it. It therefore occurred to him, that a continuous platform of iron would make as good a tie as having this tie from the top of one tower to the foot of the other, as the tie had to be anchored in each case, and the horizontal beam would quite as well do that duty, in addition to the duty of a platform as described, and the quantity of iron required to make these ties would be nearly enough to make the horizontal platform. These considerations led him to abandon what, at first sight, appeared to be the best principle, as pointed out by the Astronomer Royal, viz., that of a simple stiffening girder. No doubt there was a great reduction in deflection in the girder combined with the chain, as compared with the girder when not combined, assuming that the depth of the girder was the same in each case. In the Lambeth Bridge the platform was just sufficiently strong to support itself independently of the cables; and by this arrangement he had constructed two suspension bridges of a different kind, together making a rigid structure.

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[1] *Vide* Rankine's "Applied Mechanics," Arts. 267, 306.

[2] *Vide* Report of the Twenty-seventh Meeting of the British Association for the Advancement of Science, p. 238.

Mr. E. A. Cowper remarked that, having paid some attention to the subject of suspension bridges, and having proved and superintended the fitting up of the Kief Bridge chains, he thought it was necessary, in order to keep the chain of a suspension bridge in shape, that it should be loaded equally throughout, otherwise it would be distorted; and if it were attempted to load the chain equally, by putting a stiff platform, that platform must be as strong as a girder of the whole span of the bridge, distributing any local weight equally upon the whole of the bridge.

He would not take the case of a single weight in the centre, as that was not a usual loading; but supposing a railway train to occupy half the length of the bridge, and to be placed in the centre, it was clear that the chain would be greatly distorted; that was to say, it would be brought down in the two middle quarters, and be raised up in the two end quarters of the length of the bridge. Now, to distribute such load equally over the whole length, there must be a girder of the full span from side to side, and of a strength sufficient to distribute that central weight (equal to half the whole load that could come on the bridge) equally over the whole chain: that was, there must be a girder bridge of a certain strength, and a suspension bridge equal to carrying the whole load, combined together. It might perhaps be said, in a popular sense, that the one helped the other; but they were, to all intents and purposes, in a scientific point of view, two separate and distinct instruments: the chain, incapable of carrying a local weight without distortion, but capable of carrying an uniform loading; whilst the girder of the full span was capable of distributing a local weight equally over the whole length of that span, if it was uniformly supported throughout its length by the chain.

Although it was evident that these circumstances would necessitate a very strong and heavy girder, they were not the worst circumstances; for if the bridge were half loaded from one end, a still heavier girder of the whole span would be required to distribute the load equally on the chain, and prevent distortion. The depth of such a distributing girder would depend very much upon the degree of deflection that was intended to be allowed, irrespective of the section of metal (always supposing that it was an economically constructed girder); but in any case, it would have to be very deep. Indeed, in the Clifton Bridge it was evident that there was no attempt to make the bridge free from distortion under a full half loading from one end, the girders adopted being only 3 feet 6 inches deep, though they had the effect of somewhat stiffening the bridge, so as to adapt it for light road traffic.

He had been thus precise in reference to the stiffening of a suspension bridge by a girder or stiff platform, capable of taking a transverse strain, because he believed that that plan and his own were the only two ways that were at all admissible in a scientific and practical point of view, for the purpose of making stiff suspension bridges, or tension bridges capable of carrying railway trains.

Another plan had of late years been brought forward, with a view of stiffening to some extent an ordinary suspension bridge: he alluded to the use of diagonal ties and struts between the chain and the roadway. He had himself designed such an arrangement twenty years since, when investigating the subject of suspension bridges, but had since abandoned it, as he found that, taking into account the rise and fall of the chains in winter and summer, due to differences of temperature, it was totally inadmissible (if the platform was absolutely fixed to the piers), on account of its alteration of length; and as the bridge rose slightly (due to the contraction of the chain), one set of diagonals would be severely strained, and as the bridge was depressed (due to the expansion of the chain), the other set of diagonals would be strained, and to such an extent as to cause positive injury to them. This action might be clearly explained by reference to what took place soon after Southwark Bridge was fixed, when, on the weather becoming warmer, the arched rib rose without injury to itself, but caused some of the diagonals in the spandrils to crack, one after the other, on account of the spandrils touching the piers, and having no room to expand. The rise of the centre arch was stated to be about one inch and a half, due to temperature.

Again, if the platform of a suspension bridge were free from the piers, and had diagonals and struts, they would have to be very heavy and long, to keep the chains in shape. It was now so well understood that it was more economical to use materials with strains in the direction of their length rather than in a transverse direction, that he must beg to be excused for referring to the fact that wrought iron or steel, when used in long pieces, was far more economically employed in tension than in compression. This consideration had been strictly kept in view in the construction of the Inverted Arch Bridge (Plate 10 A, Figs. 1, 2, and 3), where every part was in tension.

Now if a moment's consideration be given to the strains that actually took place in an ordinary arched bridge, whether of stone or of iron, of true form, it would be evident that the strain passed through the very centre line of the stones, only when the arch was equally loaded throughout; but if the arch was loaded to the full extent for only half its length from one end, the curve of compression was distorted, and passed through the stones at a greater height on the side that was loaded, and lower down on the side that was not loaded.

This effect could at once be seen by hanging up a chain to represent the weight of the bridge, and the true curve of strain that passed through the stones, and then hanging on to it weights in proportion to represent the loading. Thus the true curves of strain could at once be obtained, either for moving loads or fixed loads, such as solid spandrils, &c. He had followed this plan for many years, when settling the sections of bridges, as, for instance, those at Richmond, Barnes, &c.

If therefore precisely the same method were adopted with regard to an arch in tension, instead of compression, it would only be found necessary to provide the requisite depth of wrought iron or steel in the inverted arch to include the several distorted lines of tension, in order that there might, under any circumstances, be metal to receive the tension when it came upon it, precisely as there was provided in the arched stone bridge depth of stones sufficient to include any distorted line of compression that came upon them.

Of course if the chains and roadway weighed nothing, the moving loads would distort the chains into triangular forms and short catenary curves; but it was found in practice, that for a moderate span, say 200 feet, the chains and a good road together, whether for locomotives or common road carriages, would weigh as much as the moving load. As the Inverted Arch Bridge was especially intended for carrying railways and heavy trains over large spans, the chains and road would weigh more per foot run than the moving load. The calculations, however, were all taken out as though the moving load was equal in weight to the bridge.

It might be remarked that it was quite possible to have a crowd equal to 140 lbs. per square foot. This he had proved, by placing a number of workmen in the corner of a shop, and measuring the ground they covered, and then weighing each man carefully.

Supposing that the bridge were fully loaded for one-half its length from the right-hand side, the curve of tension would be the line R, R, R, R (Plate 10 A, Fig. 2), which would be entirely within the top and bottom flanges of the inverted arch; if loaded in the same way from the left, the line L, L, L, L would be the curve of tension; if loaded in the middle, the line M, M, M, M would represent the line; and if loaded at the ends, the line E, E, E, E would show the curve of strain. All these lines were within the top and bottom flanges of the inverted arch, though in some cases closer to one or other of the flanges, and the load was only half the full load, causing much less than the greatest strain. When the strain was greatest, due to a full loading of the bridge, the strain passed exactly through the very centre of the arch, midway between the top and bottom flanges, thus dividing the strain between the two flanges.

Thus there was a line of chain in every particular form in which the curve of tension could come on it; the extreme distance between any two lines being 3 feet 6 inches for 200 feet span; the inverted arch being made 4 feet deep over all (Plate 10 A, Fig. 1); the vertical web being formed of one or more plates, and the top and bottom flanges also being formed of plates and angle-irons, as shown in section in Plate 10 A, Fig. 3; the ordinary wrought-iron cross girders being hung from the arch by suspension-rods, and, in the case of a railway bridge, the rails being laid on two whole balks of timber, or on a half balk on a light longitudinal girder.

He was of opinion that suspension bridges ought to have light side chains to the platform (in a horizontal position) in all cases of large spans, to prevent side motion being caused by the wind, much on the plan adopted by Sir Isambard Brunel, for the suspension bridge in the Isle of Bourbon.

In answer to a question, he said the light diagonals shown in the drawing were only put in to improve the general appearance, and therefore were quite optional. He considered that metal put in the platform, to enable a series of diagonals and uprights to act to keep the chain in shape, was not at all economically applied; for the strains of compression and tension had to be carried through a number of members at various angles, instead of the weight being taken at once in the true primary curve of tension, having the full depth of the versed line of the chain, as in the inverted arch.

Mr. F. W. Sheilds gathered from the remarks of Mr. Cowper, that the strains upon that construction, which might be called a rigid chain, would be calculated in the same manner as if the chain were inverted and made an arch, excepting only that the strains would be tensile instead of compressive. He would ask if that was the view which Mr. Cowper took; and if it were so, he would further ask what advantage that construction had over the ordinary arch, or bow and string construction, with which all were familiar? and whether there was any advantage to be gained by it in economy of metal or otherwise?

Mr. Cowper submitted that, having a given span and versed sine, within which a bridge was to be constructed, it was not possible to conceive a more economical method of carrying a load than that of a chain, entirely in tension. And if such a chain as was required for any load was always there, ready to receive it, it must be admitted that the load would be carried without distortion, and with the least possible quantity of metal. It only remained to notice that there would be a small amount of deflection in the proposed Inverted Arch Bridge, due to the line of tension passing from the top to the bottom of the inverted arch, or vice versâ: this amounted only to such slight alteration of form as an ordinary cast-iron arched bridge might undergo on the passing of a load.

In his original design for a structure of this kind, he proposed to have two chains, tied and braced together with diagonals; but now he suggested a vertical plate or plates, with flanges at the top and the bottom. The plan having attracted some attention, he felt bound to lay the principle before the Institution, in a general discussion on suspension bridges. He might mention that the late Mr. George Stephenson so highly approved of it, as to contemplate using it on a line then under construction, though eventually another plan of bridge was adopted.

Mr. Hawkshaw remarked that, looking to his own share in the construction of the Clifton Bridge, it might be expected that he would agree with all that had been said by the Author in praise of that structure. But Mr. W. H. Barlow knew that he did not quite agree with some observations in the Paper as to the application of suspension bridges. One object of this discussion appeared to be to ascertain to what extent suspension bridges might be used for railway purposes. Some years ago Mr. Peter Barlow gave a great deal of attention to this question, and went to considerable expense in constructing a model, the object of which was to show that a suspension bridge could be made to carry railway trains; and he investigated, with Mr. Barlow, the deflection of that model, and found that it was very great indeed. Mr. P. Barlow made alterations in, and additions to, his model, and at last obtained considerable rigidity; but the conclusion Mr. Hawkshaw then arrived at was, that to carry the same load with precisely the same deflection, it would be impossible, by any arrangement of a suspension bridge which he had yet seen, to make a bridge as rigid as a girder, except by using as much iron, or, in other words, by converting the suspension bridge into a girder.

Mr. Cowper's observations were valuable, because his plan for making a suspension bridge rigid was perhaps as good as any that had been proposed. But Mr. Hawkshaw thought, by the time the bridge had been made equally rigid, it would be found that as large a weight of iron had been used as in a well-constructed girder. If the Astronomer Royal would devote his great powers to a further consideration of this subject, it would be very valuable to the Institution. He had given the proportion between a girder not sustained by chains, and a girder sustained by chains; but he had not taken into account the sectional area of the chains. If the investigation were pursued, Mr. Hawkshaw thought it would be found, by the time precisely the same amount of rigidity had been obtained, with the same load between the points of support, that the amount of iron used would be as great in the one as in the other kind of bridge.

The application of the suspension principle, however, might be of use apart from the question of weight of material. There was no doubt that the easy modes by which chains could be carried across wide openings afforded great facilities in construction, and might render it advisable to adopt this principle, even supposing it turned out in the end that the amount of iron should be the same. The simple modes by which these catenaries might be extended across wide openings admitted of extensive application; and the chain might be converted into a girder, and scaffolding thereby avoided: and then, supposing that the same amount of iron was ultimately used, it still would be a serviceable mode of construction in certain cases. It must be borne in mind, however, that this principle was not quite new. Mr. Robert Stephenson, in his first proposition to build a bridge across the Menai Straits, proposed to use chains, and from the chains to erect the girder; but then his plan was to take away the chains. Of course that differed from the mode now suggested, which was to make the chains available for the strength of the bridge.

Mr. Sheilds said Mr. Cowper had not answered him so fully as he wished. When a bow and string bridge was equally loaded, no bracing was required; but when unequally loaded, as in the case of a railway bridge, a tie rod and bracing were required, and that necessitated an equal section of metal to what the bow required. Could Mr. Cowper get rid of the equivalent of the tie rod in a single arch, or otherwise effect any economy of metal as compared with the ordinary arch, or bow and string construction?

Mr. J. H. Porter said, having been the Contractor for the construction of Lambeth Bridge, designed by Mr. Peter Barlow, and referred to in the Paper, he would offer a remark upon the stiffening effect of the vertical and diagonal trussing, because he thought there was a great deal in the principle deserving attention. As the Author of the Paper remarked, the stiffening effect of the bridge was very considerable, as was observable on heavy loads passing over it; and that was the more remarkable, as wire cables were used instead of chains. He thought additional rigidity would have been imparted, if it had had the advantage of the rigid flat bars of the ordinary suspension bridge. He doubted if that principle could be carried out advantageously in very large spans, such as the Clifton Bridge, because of the great length of the vertical struts, and their distance from the towers; the longest struts in the Lambeth Bridge were 22½ feet from the towers, and their height 20 feet, while from the head of the strut to the top of the tower was 26 feet. The Author of the Paper had also called attention to the experiments he had made upon bars or beams of varying section, deeper at the points of support, and less in the middle of the span. He would suggest that that was a system which might with advantage be further looked into, with reference to girder bridges with diagonal trussing. He had had occasion to build several girder bridges of continuous spans, in which the sectional area of the top and bottom was increased over the points of support, and diminished in the centre; and where there were large spans there must be considerable depth of girder, and horizontal transverse stiffeners overhead; and it was also necessary to give sufficient height for the vehicles to pass beneath these horizontal transverse stiffenings. This involved the employment of much more material than was demanded by the strains produced by the bridge and its load; and he thought it likely the plan adverted to by the Author might be economical in this respect, and give a more uniform sectional area throughout the top and bottom flanges.

There was a remark made by Mr. Hemans, by way of objection to the load of 70 lbs. to the square foot, which formed the basis of calculation of the strength of the Clifton Bridge. On one occasion, he had put as many men as could hang together upon a weighing table, 10 feet long, and 6 feet 3 inches wide, giving a superficial area of 62½ feet. There were fifty-one men, weighing 65½ cwt., or an average of 144 pounds per man, occupying an area of 1·22 foot, or 1 cwt. 5 lbs. per square foot; but whether it was likely that such a packing of people would occur on any ordinary bridge he could not say.

Mr. G. P. Bidder, Jun., remarked, with reference to the combined structure proposed by the Astronomer Royal, of a roadway connected at points, in the manner described, by the suspending chain, that he understood the learned Astronomer Royal to state that, in the experiment where the roadway was simply connected with the chain in the middle, there was a deflection of 40 units; but when the chain was connected throughout the whole length, the deflection was reduced to 6 units. But it appeared to him that those figures must not be taken as representing the relative rigidity of the roadway in the two cases, because the distortion of a beam depended upon the angular deflection, not upon mere linear deflection; and where there was a larger span, the same angular deflection would produce greater linear deflection. Referring to the two experiments, in the second case, the deflection was through a space of $\frac{4}{10}$ths of the total length of the span: therefore, having in one case a deflection of the beam for a length of $\frac{4}{10}$ths, and in the other a deflection of the length of the whole span, the proportion of linear deflection must be as the squares of the span: or, in the second case, the linear deflection should be $\frac{16}{100}$ths of what it was in the other case, to represent the same angular distortion of the beam, or nearly in accordance with the actual results obtained. It followed, then, that the experiments did not indicate greater rigidity of beam in the one case than in the other.

The Astronomer Royal remarked, that was a depression which increased as the load went towards the middle, and diminished again as it approached the ends. It mattered not at all, for any one position of the load, what flexure there might be in the other parts of the beam. It was sufficient, for determining on the form to be given to the beam, to know how much the beam was deflected at the place where the load was supported. The beam was not more rigid, but was maintained by the suspending-rods, in such a shape that the point where the load rested was only depressed to the extent he said.

Mr. Bidder, Jun.—What he meant was, the stress on the beam, which was the important thing (and, therefore, the danger of fracture), was not represented in the two cases by the proportion of the respective linear deflections. Although the deflection might be little in linear measure, it might, on a shorter beam, represent greater distortion and more danger than where a greater deflection was got upon a longer span.

The Astronomer Royal said he mentioned two things. The flexures produced by the load were not such as to put the beam in danger; but these were compared with the flexures of the same beam, carrying the same load, and supported at small intervals; and that was the only way in which the rule was obtained on which he proposed the construction to be adopted in future cases. Using the same beam of which the deflections were ascertained when it was supported by a chain, that same beam had been the subject of experiments when supported at different intervals, without any chain suspension. He said, it had been suggested, with reference to his communication "On the use of the Suspension Bridge with Stiffened Roadway," that it would be necessary to take into account the weight of the chains proposed for supporting a very wide bridge; and that the quantity of iron, thus increased, might amount nearly to the same as that required for a simple tubular bridge of the same great width. A very simple calculation, on an assumed numerical instance, would show that this was by no means the case. Supposing that (using the proportion which the Author inferred from experiment) a span of 1,200 feet was to be crossed by a tube, such, in section, as would commonly be employed for a span of 400 feet; and supposing that the chains, which assisted to support it, left the piers at an angle of 30° to the horizontal (which was nearly the customary angle). The tension of the chain at those points would then be the same as the entire weight to be supported, including tube, suspending-rods, load, and chain. That part of the tension, therefore, which was produced by the weight of the chain alone would be equal to the weight of 1,200 feet of chain. But the strength of the chain, he believed, would carry, at the breaking-point, the weight of 16,000 feet of chain, and might be trusted with the weight of 3,600 feet, or three times the weight just considered. Therefore, if the full permissible strain were put on the chain, the weight of the tube and suspending-rods, &c., would be twice as great as that of the chain; or the quantity of iron in the chain would be one-half of that in the tube, &c.

In this instance, the whole quantity of iron employed would be $\frac{3}{2}$ that of a simple tube whose section was the same which would be used for 400 feet span. But if an attempt were made to cross a span of 1,200 feet by an unassisted tube, he professed his inability to conjecture what its section would be. Perhaps no living Engineer would attempt it: it must, at any rate, have great breadth, enormous depth, and formidable bracing. There could be no great error in saying that its section would require five or six times the quantity of iron which was sufficient for a tube of 400 feet span—instead of $\frac{3}{2}$, when the assistance of chains was used.

Again, supposing a span of 300 feet, to be crossed by a tube whose section was that proper for an extent of 100 feet. The auxiliary chain would support safely twelve times the tension caused by the simple weight of the chain, and would therefore be able to support a tube whose weight was eleven times that of the chain; or the iron in the section of the chain would be only one-eleventh of that in the section of a tube proper for 100 feet span. There could be no remaining question, therefore, on the enormous saving of iron produced by this composite construction. The explanation of this was very simple. Of all known methods of supporting a projecting weight, the use of the tube was, mechanically, the most disadvantageous, and the use of chain the most advantageous.

With reference to the last sentence of his communication, he might, with the assistance of some of these figures, point out how easily the numerical calculation might be made. Supposing, in such a bridge as this, the chain left the pier at an angle of 30° with the horizon, or 60° with the vertical. Then, considering how the entire weight of the bridge was supported, the strains at the two suspending points of the chain making an angle of 120° between them, and the direction of the weight of the bridge making an angle of 120 with each of them, it would be seen that the tension which the chain sustained was equal to the weight—not more nor less; and if an accidental shifting load was put upon the bridge, the tension which it put upon the chain would also be its weight, except so far as the position of the chain might be a very little shifted; but, generally speaking, the tension of the chain where it left the pier, and where the tension was greatest, was equal to the entire length and load to be supported. That was one form of construction. Now, comparing that with the circumstances of a tube such as that of the Britannia Bridge; and he supposed that he should not be far from the truth in saying that the two points of action, namely, of tension in the lower part, and thrust in the upper part, might be 25 feet apart; and in round numbers—or rather square numbers—he might take the length as 400 feet; that was, sixteen times the depth of the tube between the two acting points of thrust and tension. It required but a few moments' calculation to see that, in that case, the tension at the bottom of the tube was four times the weight to be supported; the thrust at the top of the tube was also four times the entire weight to be supported, supposing the weight to be equally distributed. Therefore it would be necessary to provide sufficient strength, in the construction of the bottom and top of that tube, to support more than eight times the entire weight to be supported; and, observing that thrust required a greater quantity of strength of iron than tension, he might say, as strictly correct, that the entire quantity of strength to be given to the top and bottom of that tube must be such as would be required to carry nine times the weight of the tube. In this statement, the weight of the load was supposed to be equally distributed throughout the length of the tube; but when a travelling load, such as an engine and train, came to the middle of the bridge, eighteen times the weight of that must be taken. Therefore, this was the contrast between the two states of things:—that where there was suspension by chains, the tension on the chain, and the strength required in the iron of it, were equal to the weight of the entire load to be supported, or thereabouts; but when a tube was used, the quantity of strength to be given in the aggregate to the top and bottom must be about nine times the entire weight supposed equally distributed, and eighteen times the weight of the accidental load which was put upon the middle. He was confident, after that, that there were better modes of crossing a wide span than by a tube; and he thought Engineers had been dazzled by the great success of the Britannia Bridge.

Mr. C. B. Lane wished to offer a few remarks on the question of 70 lbs. per square foot as the load for bridges for the use of ordinary public traffic. He would show, by figures, what were the weights and areas that a crowd of men would represent on the average, thence deduce the maximum weight which a superficial foot might have to sustain, and then trace the peculiar circumstances and conditions under which that maximum might be approximated to, if not actually attained. The average weight of a number of men was about 140 lbs. each—the girth of their bodies, at the largest part, about 36 inches; in fact, it would require a man above the average, or about 38 inches, to have a diameter of one foot, supposing his figure to be cylindrical; the area he could occupy would be, therefore, ·7854 of a superficial foot; but looking at the fact that the average was 36 inches, he occupied about ·71 of a foot: on that space, then, 140 lbs. might be placed, or at the rate of 187 lbs. per foot superficial. In France, Engineers considered 50 lbs. to 60 lbs. to be the maximum weight per foot that could in this way be brought upon a structure, but the whole tenour of French legislation for centuries past had been unfavourable to the meeting together of large masses of men: every one knew that, wherever a large body of people collected together in that country, the agents of the police immediately exercised a dilating, or centrifugal force. The condition of things in England was entirely different: people had the liberty of meeting in as large masses as they chose. There were also in this country, and especially in London, unexampled facilities for the meeting together of great crowds of persons. Supposing a few years hence, when the Metropolitan Railways were completed, the Houses of Parliament to be on fire,—and he had a right to suppose that what happened once might happen again,—could any one fancy what would be the crush on all the bridges in the vicinity of Westminster? The agglomeration of human beings would probably approximate closely to the before-mentioned maximum. Over and above the consideration of the statical weight of a mass of human beings placed in that way, there was the fact that in dense crowds there was always a great deal of surging, and consequent *vis viva* generated, which ought to be allowed for. In proof of this, he had only to call attention to two accidents which had occurred to suspension bridges within the memory of many persons now living—one at Yarmouth, in the year 1845, where, from the surging force produced by a dense mass of people, the bridge gave way, and about one hundred and twenty persons were drowned; the other in the south of France, where a bridge broke down when a regiment of soldiers was marching over it, and a considerable number of lives were lost.

He thought, looking at these figures, which any one might verify, and these facts, which were historical, that 70 lbs. to the superficial foot was an insufficient datum to calculate as the probable maximum weight which bridges (at all events in the vicinity of London) might be called upon to sustain.

Mr. F. J. Bramwell said it had fallen to his lot to have seen many of the largest suspension bridges in various parts of the world. He had seen the temporary bridge over the Niagara River, erected before the railway bridge was built, and he had also seen, in the year 1853, the suspension bridge over the same river, about 6 miles lower down, which united Queenstown on the Canada side, with Lewistown on the American side. This bridge appeared to be but little known, although he believed it had the longest span in the world, being 1,040 feet between the supports of the ropes, in the rock, and having a length of roadway of about 853 feet. The bridge was carried on each side by a group of five wire ropes, of $2\frac{3}{4}$ inches diameter, measured outside a species of "serving." These ropes were about 8 inches apart from centre to centre, making about 2 feet 8 inches from centre to centre of the outside ropes of each group. The five ropes were connected at intervals of 20 feet $1\frac{1}{2}$ inch, by ties and stretchers. The suspension rods were of flat iron, $\frac{3}{4}$ inch thick, and 4 feet $\frac{1}{4}$ inch apart from centre to centre, and the roadway was 20 feet wide.

The two bridges at Fribourg had been alluded to, but it had not been mentioned that one was not uniform in its span, that was to say, the lowest point of the curve, instead of being in the centre, was about two-thirds or three-fourths of the way across. He believed if the greater length from the lowest part of the curve to one of the supports were regarded as the half span of an uniform bridge, it would be found to be by far the longest span in the world.

With regard to the temporary Niagara bridge, it was, in his opinion, one of the best instances of the successful attainment of the ends to be fulfilled by cheap and simple means. He regretted he had not taken the particulars of that bridge, but he well remembered the extreme lightness of the structure, at the same time coupled with a stiffness that was sufficient for all the purposes of the traffic—sufficient, in fact, for the heavy omnibuses that passed over it from the railway station on the American side, to the hotels on the Canadian side.

He could not help thinking, in spite of what had fallen from the Astronomer Royal, that the true use of the principle of suspension was to bridge across large chasms by the most simple means, and for traffic where a considerable amount of deflection in the bridge was not a matter of importance. There was one thing the Americans did which it might be well to copy: that was, the ropes which carried the bridge, instead of being in one plane vertically from end to end, were far wider apart at the saddles than at the roadway. In the Queenstown Bridge, before alluded to, the ropes at the centre were only of the width of the roadway apart, viz., 20 feet; but at the saddles he believed they were 54 feet apart; and in that way great lateral stiffness was given to the bridge. If Engineers devoted themselves to the introduction of improvements such as these, and to other improvements in building suspension bridges for the purposes for which they were originally designed, he thought they would do better than by spending their time in the endeavour to make them suitable for railway trains. As regarded the efforts to make cheap and light bridges for common traffic, he did not think any one should be deterred from this course by the statement that had been made, about the facility with which considerable vibration was set up in a bridge of this kind; because it must be borne in mind that, with a true suspension bridge, the stronger the material employed for the rope or chain, the less its weight, and, therefore, the greater the vibration. If, for instance, steel wire, equal to a breaking strain of 100 tons to the square inch, were used, there need clearly be much less weight of chain, and, therefore, less resistance to the effect of a passing load, than if a low quality of iron, equal to a strain of only 20 tons to the square inch were employed; and, therefore, a bridge was more or less 'lively,' in the inverse ratio of the quantity of material in the rope or chain. No doubt there was great temptation, when all that was required could not be got by one means, to try to obtain it by others. An hour-glass was in itself a convenient means of noting time; but if it were desired to make it turn itself and strike, by the time machinery had been provided for that purpose, the probability was a very complicated and bad kind of clock would be the result. He thought this was a parallel to the case of stiffened suspension bridges; and, to his mind, this was shown by what had been advanced by the Astronomer Royal, as to the direction in which efforts should be made to give to a suspension bridge the stiffness which some people aimed at. The case of the Lambeth Bridge had been cited: if that bridge were now under discussion, he would be glad to enter into that point; but it was not. He certainly thought Mr. Hawkshaw put the matter very clearly when he said, by the time as much iron was employed as would make a suspension bridge perfectly stiff, it would be found as much material had been used as would make a girder bridge; and he thought that was confirmed by the arguments of the Astronomer Royal himself, and for the following reasons:

Referring to two diagrams, Mr. Bramwell said one was intended to represent the principle of a suspension bridge with a girder underneath to stiffen it; the other was the same in principle, but the girder was raised until it intersected the chain at two points, each one-third of the span from the end: under these circumstances, the middle piece of the chain might be dismissed from consideration; but when that was done, the effect was to produce an inverted queen-truss without the top member. Supposing that to be so, the span was then broken up into three equal parts, each carried by a girder one-third of the full span; and then without any assistance from the chains, the girders being each only one-third of the length of the total span, the deflection would be only $\frac{1}{27}$th part. Now it was said by the Astronomer Royal, that if a stiffened roadway was connected with a suspension chain, the deflection produced by a given weight would not be more than $\frac{1}{26}$th part of that which would be produced by the same weight on the chain unconnected with such a roadway. Therefore it appeared to Mr. Bramwell, that when the girder was supported on three points, and even if the three lengths were fastened merely by pins, so that the girder was not continuous, there would still be only the decreased deflection spoken of. Looking at this as an inverted queen-truss, it remained to show how the diagonal ties represented by the remainder of the chain (the central portion being taken out) might be carried. If left with a curvature upon them, it might be said the bridge was not rigid. This might be got over in two or three ways: one was, by the plan of the over suspension chain, adopted by Mr. Ordish to take the weight of the ties; another was, to keep them to a more acute curve than they would naturally assume, by means of diagonal rods going down to the corner, where the roadway joined the piers; or the suspending rods might be trussed, as was done in the American bridges. In the case of a bridge of a single span, with a hold on the rocks at each end, a top member was not required. If there was no hold on the rocks, then, in a single-span bridge on Mr. Ordish's principle, the top of the supports, the piers, were prevented from coming together by means of back stays; but if there were a number of spans, it would be necessary to have a top member, or something to represent it, in order to prevent the tops of the piers from coming together; and in that case the bridge would be a reproduction of Mr. Brunel's Chepstow bridge, or an inverted queen-truss. This went far to confirm the views of Mr. Hawkshaw, that, by the time an amount of stiffness had been put into a suspension bridge, sufficient to make it practically rigid, as much metal would have been expended as would have made an absolute girder bridge.

There was one other point only to which he would refer, and that was, the mode in which he had endeavoured to convince himself—by the eye rather than by figures—that a girder deflected according to the cube of its length. For illustration—he would take a common hinged rule, which, when slightly bent at the joint, might be taken to represent a girder one foot long, and deflected by a given weight to a certain amount; if now the length were increased to two feet, the angle of the parts of the rule remaining the same, the deflection would be doubled, because there would be double the length of the same slope; but with a given weight on double the length, the angle made by the two parts of the rule with the horizontal line would be doubled, and this increase, or doubling of the slope of each part with double the length, would give four times the deflection ; this was, however, supposing that the deflection occurred at one place only, and not, as in practice, throughout the length. But as the deflection occurred throughout the length, and as in a girder of double length there were double the number of particles to be deflected, the total deflection, if the length of a beam be doubled, would be eight times that of the original beam.

This was a rough way of proceeding, but it was one that could be readily followed and understood.

Mr. W. R. Kinipple gathered from the course the discussion had taken, that rigidity in suspension bridges was the great desideratum. If so, a rigid bridge should be constructed at once, and to do that, it was only necessary to take Mr. Barlow's platform, the Astronomer Royal's system of suspension, Mr. Peter Barlow's bracing, and Mr. Cowper's inverted arch, and the result would be a bridge, turned upside down, in many respects similar to the Victoria Bridge at Pimlico. He found that such a bridge as that, would take about $\frac{7}{10}$ths of the metal required for an ordinary tube. He considered it better to make a continuous cantilever tube of sufficient strength between the piers to carry itself and 33 tons in the centre. By dividing such a tube into three equal parts, it would then be equal to 100 tons in the centre of each bay, or 600 tons spread over, and by providing braced chains or stays in position of sufficient strength—say 200 square inches sectional area—a direct pull could be obtained, and the catenary curves, which gave rise to the great evils of oscillation, be got rid of. It was clear that the expansion and contraction of the right-angled triangle formed by the roadway, the wrought-iron tower and the suspension chain, would not disturb the state of equilibrium. The only question for consideration was the longitudinal expansion and contraction similar in every respect to an ordinary tube, having sufficient rolling surface at the points of support on the piers. He believed such a bridge would be in any one part quite independent of any other part; and that a weight placed between the centre and a pier could not cause deflection, because the lattice girder would be of sufficient strength to resist deflection, thus preventing any oscillation or wave motion in any other part of the tube.

Mr. T. Marr Johnson had not much to say upon suspension bridges; but, as he was well acquainted with the design referred to in the Paper by the Astronomer Royal, he would say a few words in connection with it. He assumed that the practical test of the merits of two designs was in their comparative strength and stiffness when the span, load, and weight of metal were the same in each. He would take the design above referred to, calling it the bridge with stiffened chain (or No. 1), and compare it with the design of the Astronomer Royal, as shown by the model, calling it the bridge with stiffened roadway (or No. 2). Assuming the same weight of metal in each, and proportioning the parts of the latter (No. 2), as indicated, he found that, whereas the bridge No. 1 would sustain a deflection by a passing train of about 5 inches, and this in a continuous curve from end to end, when the same train was passed over No. 2, there was a downward deflection on the loaded end, and at the same time an upward deflection on the unloaded end of the bridge, the difference of level of the two extremes of curvature being about 15 inches. The position of these curves would, of course, change with the progress of the train; thus a wave was created, and great unsteadiness, not only during, but some time after the passage of the train. Besides this objection of unsteadiness, the girder for stiffening the roadway was exposed to strains from change of temperature and consequent alteration in the length of the main chains, which in this climate would increase the pressure on a square inch of the metal of the girder from 5 tons to 7½ tons.

He thought that stiffened suspension bridges were generally designed with too little regard to the effect of the changes of temperature; the stiffening tending to make them as rigid as a girder without the same allowance being made for expansion, &c. He alluded chiefly to bridges of very large span. Take for example Fig. 4, Plate 10 A, which showed an arrangement under the roadway of the form of an inverted suspension bridge with vertical rods. If the temperature were reduced, both chains would be shortened and the ties be fractured; if raised, both would be lengthened, and the bottom one would become slack and useless as a stiffener.

The design with inverted arch of an I girder section was objectionable, for, in the case of its lengthening by heat, the bottom flange would have all the load to carry, besides a strain resulting from the compression of the top flange; and the reverse would take place in cold weather; so that both the top and the bottom flanges would have to be strong enough to carry the whole load the bridge was intended to sustain, and no economy would result.

He could not agree with the proposition that the same quantity of metal must be expended in a suspension bridge, to obtain the same stiffness, as in a girder. There was great economy in the suspension bridge, because the bulk of the iron was in tension, and in that condition was capable of sustaining a much greater load than when in compression, and particularly so when, as in a trussed bridge, the compression members for a large portion of their length were acting as unstayed columns.

He would show how the metal by the stiffened suspension principle was saved, in comparison with a bridge on the Saltash principle, leaving out for a moment the question of thermal expansion. The top tube of the Saltash bridge was placed to act as a stretcher, keeping the piers (or rather the ends of the tension member) apart, as well as to transmit the strains communicated by the bracing from one part of the bridge to the other under an unequal load, such as the passing of a train; in other words, to stiffen the bridge. Now if the stretching part of the tube were taken away, and the back chains were used to do the same work, enough of the tube being left to effect the stiffening only, a weight would at once be taken off a bridge, of say 750 feet span, equal to the weight of the maximum test load; so that not only would the iron and the cost of it be saved, but the strains brought by reason of its weight upon the chains and upon the bracing would be got rid of, saving metal in each, and the only metal to be placed on the other side of the account would be that in the back chains; in short, for a bridge of 750 feet span (design No. 1), there would be a saving of no less than three-eighths of the metal employed, taking in both cases the same strain per inch for the same span and load. This, however, left the bridge without arrangement for expansion, as it was fixed at both ends, and it became necessary to divide the stiffening into two parts, as in design No. 1; making, in fact, a joint in the centre of the bridge, and thus allowing vertical motion for adjustment to change of length, in which there was no inconvenience. The land chains would, of course, be maintained in the form of a rigid curve by downward ties, or trussed to straight lines, as might be convenient. In this way perfectly stiff bridges of 1,500 feet span might be safely constructed with great economy, especially if steel were used. He suggested that experiments with that material should not longer be delayed, for, until it was decided what steel could safely bear, he did not think that bridges of very extraordinary spans would be constructed in this country.

Mr. Price Williams observed, in reference to the remarks as to the necessity that existed for further and more conclusive experiments on the strength of steel, and as to the desirableness of the employment of steel in the place of iron in bridges of this construction, that a series of interesting experiments were now being carried out by some members of the Institution, the result of which, he hoped, would be to supply the requisite data upon which Engineers might safely depend in using this material, in circumstances where the utmost strength and economy of material were essential. He was much struck with an observation in the Paper to the effect that the Author had seen reason to doubt the same advantage in the use of steel in compression. Mr. Williams was quite aware that if there were few conclusive experiments on the tensile strength of steel, there were still fewer in regard to its compressive powers. However, in such experiments as had come under his notice, he did not mark that absence of resistance to compression which had been spoken of; and he felt satisfied, from a practical acquaintance with the manufacture of steel—both Bessemer and ordinary steel—that the results of the experiments now being made would prove that its power of resistance in compression was equal, if not greater, than in tension. Colonel Wilmot, in his experiments on Bessemer steel, at Woolwich, found the ultimate tensile strain averaged 78 tons, while in compression it averaged as much as 95 tons to the square inch. Again, Mr. Berkley, in his experiments on the compression of small cylinders of about $\frac{3}{4}$ inch diameter, and 1 inch high, showed that with 18 tons per square inch there was no indication of permanent set; and that after subjecting the cylinders to a pressure of between 50 tons and 60 tons to the inch without failure, he considered there was no object in carrying the test farther. Experiments of his own at Mr. Kirkaldy's, upon hard Bessemer steel, showed surprising powers of resistance, both to tension and compression. He ventured to state that, with the increased knowledge and experience in the manufacture of Bessemer steel, it was found that by very slightly increasing the percentage of carbon from about ·45 per cent. (the limit for the softer descriptions of steel) to about 1·25 per cent., a class of steel could be obtained almost analogous in its resistance to compression to white cast iron, with this difference, that whereas in the cast iron the tensile power was very low, in this description of Bessemer steel a very high tensile was combined with a high compressive power. In his opinion, the proportion of carbon in the steel was an important element in the experiments now being carried out, and he ventured to suggest that this should not be lost sight of; but that an accurate analysis should be made, and a record kept, of the exact percentage of carbon in each specimen experimented upon. If this course were adopted, and supposing due regard to have been shown by the manufacturer to a judicious selection of the crude material, he saw no reason to doubt the result would be that a material could always be obtained fully equal to the utmost requirements, whether for compression or tension.

Mr. Fowler, President, expressed a hope that the results of the experiments would be made known at an early day; as, he said, nothing was more interesting than exact facts with regard to the compressive power of steel.

Captain Tyler would take the opportunity of saying, how glad he was to learn that experiments on the question of the application of steel to railway bridges were still going on; and he hoped some day to see that material used in a good railway suspension bridge. He did not think it so important as a practical question to ascertain the power of steel in compression. It was more important to know to what extent it might be depended upon in resisting a tensile strain, and how far it might be got in the market of a homogeneous quality. It would hardly be economical to use steel in compression in railway bridges. The great problem, in employing a material of high power in the parts of a girder which were subject to compression, was so to dispose it as to prevent them from buckling. He recollected going to Liverpool, four or five years ago, to witness the testing of a puddled steel girder of which much was expected. It was made at the works of the Mersey Steel and Iron Company. It failed under a weight less than a wrought-iron girder of analogous dimensions would have supported, and principally because it was unable to resist the effects of torsion and buckling. That was one point in regard to which, in the future use of steel, care must be taken. But the question now before the meeting was, not so much whether steel could be utilized in a railway suspension bridge, as whether it was justifiable to construct a suspension bridge for railway purposes at all. Mr. Hawkshaw had expressed an opinion, that in order to stiffen a suspension bridge so as to reduce its deflections to those of an ordinary girder, as much metal would be required as to construct a girder. Mr. Bramwell had illustrated that argument by saying that, if an upper member, such as a tube, were added to keep the piers asunder, then something very like the Chepstow bridge would be arrived at. But Captain Tyler would say in reply that if, on the other hand, that top member were not added, there would then clearly not be the same weight of iron as in the Chepstow bridge. It was plain, that whenever it was possible, instead of adding such a tube between the tops of the piers, to tie them back by means of chains, without being obliged to provide for the support of the great weight of such a tube, much the same result could be obtained as in the Chepstow bridge, or in the case of any other similar iron girder.

In discussing this subject some years since with Mr. Peter

Barlow, he had been much struck with the views then advocated in regard to the stiffening of suspension bridges. He had also been glad to see them since carried out, as an interesting experiment, in the Lambeth bridge. At the same time, he did not think the principles which had been there put in practice were well adapted for a railway suspension bridge. In very long spans—for which railway suspension bridges were required—the struts would become too long towards the ends of the bridge, and the suspension chain did not afford a stiff member for them to strut against. Then, with regard to the proposed Inverted Arch Bridge, Mr. Cowper had explained the principles of such a bridge by reference to a stone arch; but that was hardly a practical or a fair comparison. The inverted arch, which was really a hanging girder, might more properly be compared with a cast-iron or wrought-iron arch—say of 600 feet span. The strains in the hanging girder would be the converse of those in such an arched rib. When such an arch was unequally loaded with a train of 200 tons or 300 tons, it would require a great deal of stiffening; and, indeed, the important point in all cast or wrought-iron arch bridges was not only to put enough metal into the ribs to resist compression under an equally distributed load, but sufficient metal, properly disposed, to enable the arch to retain its form under varying loads, and to act, in doing so, partly as a girder and partly as an arch. In like manner it would be necessary, in the case of the inverted arch or hanging girder, not only to employ sufficient metal to sustain the weight as a chain, but also to add as much as was required to stiffen it as a girder. This process might easily be carried to so great an extent as to realize Mr. Hawkshaw's objection. As much metal might be required as would make a hanging girder of that awkward shape, added to the back chains, platform, and suspension-rods, nearly, if not quite, as heavy as an ordinary girder for the same span. Messrs. Ordish and Le Feuvre's bridge was most ingeniously contrived, but he hardly thought it was adapted for railway purposes. It was under considerable disadvantages when acted upon by extremes of temperature, which was an important question in all suspension bridges of great length; and heavy moving loads would, as they came upon different parts of it, bring certain members into strain in a sudden manner, which would, he thought, be objectionable.

The principal problem to be solved in dealing with a railway suspension bridge as compared with a road suspension bridge was, to adapt it to support concentrated weights moving at high velocities, and for that purpose a great deal of stiffness was required, not merely to resist deflection in the centre of the span, but particularly at certain points between the centre and the piers, in cases of unequal and constantly varying loads.

He would suggest a mode of construction which he thought possessed advantages, and would meet the case (Plate 10 A, Fig 5). He proposed to place lattice girders over the piers, tied down at the shore ends as well as to the piers. The piers might be at one-third of the length, or at any convenient distance from the shore ends, on both sides. These girders, counterbalanced or tied down, would afford all the advantages required of stiffness, without causing any or much stress upon the suspension chains. The two girders thus placed, self-supporting, on each pier, would leave a space between them, which should be filled up by a lighter girder, carried by the suspension chains, and properly connected with the principal girders, so as to allow of expansion and contraction at its extremities. The principal girders, besides affording a stiff platform for varying loads, would also firmly hold down, and thus prevent undue movement in the suspension chains, and they would do so the more effectually by being tied down to the piers. The chains could not lift between the centre and the piers, and could not, when thus held down, deflect unduly in the centre. By these means a very stiff bridge indeed might be obtained, adapted for expansion and contraction, and for varying loads at high velocities. He ventured to suggest that this principle of construction would be found superior, for the purposes of a railway suspension bridge, to any of the many interesting and varied forms of construction which had been described, and particularly in cases in which two side spans in addition to the central span, were required.

Mr. Edward W. Young said the back tie of the Clifton Bridge had a flat curve, and was neither supported by struts nor held down by ties. He imagined the straightening of this chain would add to the deflection of the bridge when loaded. He thought the deflection of the bridge when loaded might be diminished, if the land-chain was either supported so as to form a straight line by vertical struts, or held down to its curved form by vertical suspension rods anchored to stones in the roadway. Another slight diminution of deflection might be produced by keying up the land-tie at the point where it emerged from the ground or rock, so as to put an initial strain upon the chains between that point and the bottom of the anchor. There would then be no depression caused by the stretching of that portion of the chain. He did not know what proportion of depth for the horizontal girder had been adopted, but it seemed to him too shallow to effect rigidity. His own idea was that the proper depth for the horizontal girder would be $\frac{1}{12}$th of the half span. Supposing the horizontal girder to be jointed at the centre, which was generally admitted to be the proper thing, the horizontal girder formed an S curve with the point of contrary flexure at the centre of the span, and the load upon that half span was one-fourth of the whole moving load distributed over it: therefore he considered it should be treated as a girder of a length equal to half the span of the bridge uniformly loaded with one quarter of the whole moving load. Since every little helped, there would be less deflection if the land-tie were made of plates riveted together, and supported by one or two struts, as by this means the average section of the metal would be increased, though the ultimate strength would be the same. He did not know the object of making the roller bed on the top of the piers at an inclination. Was the object to diminish deflection by the rising of the ends of the chain when the load came upon it? He considered the purpose to be attained was to convey the load safely from one side of the ravine to the other. He did not know whether it was necessary in such cases that there should not be more than a certain amount of deflection. It was, he thought, a pity not to adopt the use of suspension bridges for railway purposes, because there was more deflection in them than there was in the girder system. He agreed that to make the deflection the same, about the same quantity of metal would be required in the suspension as in the girder bridge. If a straight strut were put between the towers of the Clifton bridge instead of back-ties, it would answer the same purpose; but there would be more metal to carry, and larger chains would be required to carry it: therefore the deflection would be less, because there was so much more metal in the structure. He considered that the Inverted Arch Bridge corresponded with the top member of a bow-string bridge. A rigid bow-string bridge might be erected without any diagonals, by making the top flange a box-girder and the bottom tie of considerable depth, as was often done. What he proposed to do was to utilize the transverse stiffness of the top and bottom members: with a slight addition of metal to them, they, being connected together by vertical rods, would offer sufficient resistance to unequal loading to allow bracing to be dispensed with. The bridge he advocated might be described as a compound cantilever and truss bridge. The centre part was a truss, and was supported by cantilevers. The advantage of this was that the great bulk of the metal was employed in supporting the load, and very little was used to resist distortion. In suspension bridges a great quantity of metal was put in to resist the effects of distortion. The reason he believed this description of bridge would be cheaper than an ordinary suspension bridge was this—that in a suspension bridge, when stiffened, the weight of the metal was pretty uniform throughout the span. In a bridge of the description now referred to, the metal was not uniformly distributed throughout the span. There was comparatively little metal in the centre of the span; the great bulk was concentrated at the piers, and that in large spans made a great difference. He had compared this with the ordinary

SUSPENSION BRIDGES.

Fig: 1.

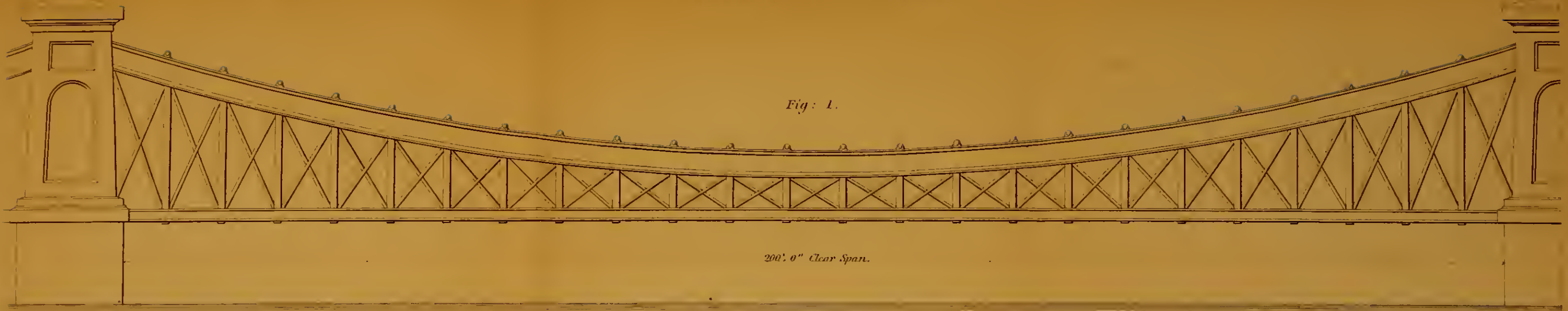
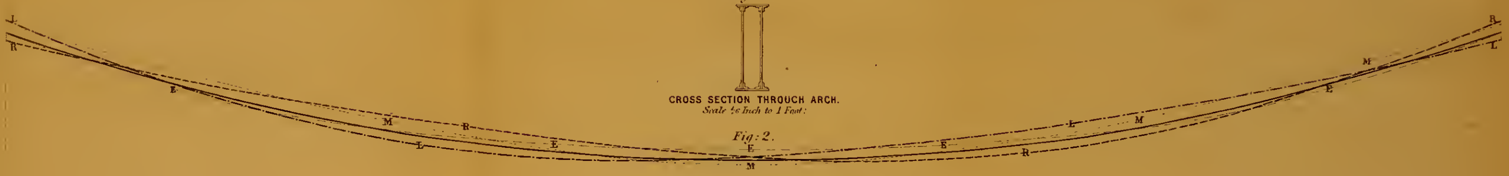


Fig: 3.

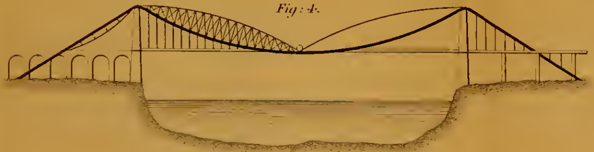
CROSS SECTION THROUGH ARCH.
Scale 1/6 Inch to 1 Foot:

Fig: 2.



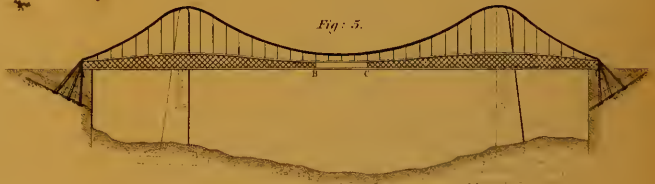
INVERTED ARCH BRIDGE.

Fig: 4.



DIAGRAM, SHEWING A BRIDGE WITH STIFFENED CHAINS.

Fig: 5.



Scale for Fig 1.

Scale for Fig 5.

unstiffened suspension bridge, and he found it took less metal. In a bridge of this description which was about to be erected in Russia, the dimensions were as follows:—

	ft.	in.
Centre span	226	0
Two side spans, each . .	81	$9\frac{1}{2}$
Width	35	0

Moving load, 120 lbs. per square foot of roadway.

Metal required . . 140 tons wrought iron
70 tons cast iron
20 tons cast iron in towers

Total . . 230 tons, for a length of 389 feet 7 inches by 35 feet wide.

Mr. G. H. Phipps felt much interested in the Clifton bridge, in consequence of having been connected with Mr. Brunel, some twenty-seven years ago, in designing the proposed iron work of the original bridge. In those designs there were many novelties, particularly a method proposed for relieving the rollers and roller-beds from the pressure upon them, to be used in case of either of those parts becoming worn or broken, when new ones might be applied. The apparatus in question consisted of one or more hydraulic presses to be fixed on the top of the suspension tower above the level of the saddles. The upward motion of these hydraulic presses gave motion, by means of strong links placed nearly horizontally, to levers so arranged as to draw upon the suspension chains in the direction of their length, and thus to transfer the tension on the chains from the ordinary connecting pins to the levers above, when the pins might be withdrawn, and the rollers and roller-beds would be free for inspection or renewal. He believed that Mr. Brunel was the first Engineer who designed and executed the moveable saddles on the tops of the suspension towers, arranged with series of rollers running upon flat roller-beds, and carrying flat plates above, on which the links of the chains rested. This Mr. Phipps considered was a very superior plan to that made use of in the Hammersmith Bridge, and some others, in which curved suspension links passed over fixed rollers, which would consequently, if pushed too far, throw a certain amount of transverse strain upon the links, and divert the strain upon the piers from its true vertical direction. On Mr. Brunel's system, where the roller-beds were horizontal, it was clear that no pressure except a vertical one could come upon the tops of the towers.

Passing from matters of the past, and coming to the subject more immediately before the meeting, he would say, with reference to the remarks made in the Paper upon the means of rendering suspension bridges sufficiently rigid for railway purposes, that he had at various times given considerable attention to that question. One of the earliest suggestions for this purpose was made by Mr. Tredgold, who threw out the notion of constructing the chains of suspension bridges of cast iron, the increased weight of which, in proportion to the moving load, would have the effect of diminishing the amount of vertical motion. Referring to another system, he had made several years ago, while the Act for the present Victoria Bridge across the Thames was in Committee, a design for a bridge for that situation consisting of suspended catenary tubes of boiler plate. He, however, found, after it was completed, that Mr. Cowper had suggested a similar construction at an earlier date. The defect of this system, as it appeared to Mr. Phipps, was the difficulty of getting the catenary tubes into their places; a difficulty, however, which Mr. Cowper did not apprehend. The quantity of iron in such tubes would be about one and three-quarters as much as that required for a simple-jointed chain of the ordinary construction. This excess was necessary for the purpose of obtaining rigidity; but since all attempts in that direction were attended with the cost of extra material, the comparative cost of this and other methods was the real point for inquiry. He had been informed that there was to be seen at Vienna a suspension bridge, in which rigidity was obtained by having two chains on each side of the bridge, hanging in similar catenary curves one above the other. These were connected together by vertical pieces at the joints, and were braced by diagonals across the enclosed spaces. This plan appeared to him to fulfil the conditions aimed at by the suspended catenary tubes, at the same time doing away with the difficulty of erection possibly connected with that system.

The next method for obtaining rigidity, to which he would refer, consisted in placing a continuous tube or pair of longitudinal girders beneath the chains for the whole length of the platform; the work done in variously deflecting the tube or girders tending to correct the mobility of the chain. The largest example of this system, he believed, was in Mr. Roebling's remarkable bridge at Niagara. Mr. Page had adopted the system in the Chelsea bridge, where two strong longitudinal girders run for the whole length of the bridge in the lines of the suspension rods. Mr. P. Barlow had also carried out the same design in the Lambeth bridge; but, in addition to the longitudinal girder, he had introduced a system of diagonal trussing between the girders and the wire cables.

He would now proceed to the examination of three points in connection with the design of the Clifton Bridge:—

First. What amount of vertical motion would the platform of the bridge undergo under the condition of one half only of the suspended platform being fully loaded, keeping out of sight for the moment any modifying effect of the continuous longitudinal girders 3 feet deep?

Secondly. To what degree would longitudinal girders modify the vertical motion?

Thirdly. What alteration must be made to the existing bridge, supposing it were required to be rendered suitable for a railway bridge, if stiffened by deep longitudinal girders?

In answer to the above queries, he had calculated approximately:—

First. When the bridge was fully loaded over one half of its span only, that the rise and fall of a point intermediate between the centre of the bridge and the suspension tower would be about 4 feet.

Secondly. That this quantity of motion would not be practically affected by the action of the existing longitudinal girders. When deflected 4 feet between the centre of the bridge and the towers, these beams would have their upper and lower booms strained up to about 5 tons on the inch, to effect which a load of only about 8 tons would be required; a force too insignificant to modify practically the deflection above mentioned.

Thirdly. In order to render the bridge suitable for railway purposes, he thought the first thing to decide was evidently the amount of deflection which might be permitted in such a case between the centre of the bridge and the towers. This he had taken at 6 inches. Theoretically, the deflection due to the effect of unequal loading upon a suspension bridge might be corrected to any required degree, by the attachment of a sufficiently deep and strong continuous longitudinal girder to the platform of the bridge. The requisite conditions were simply, that when the girder was deflected into a curve of contrary curvature, as it would be under the presumed unequal distribution of the load, the upward action of the depressed portion being deducted from the weight of the loaded half of the bridge, and the downward action of the elevated portion of the girder added to the unloaded half of the bridge, the final ratio between these pressures on the two halves of the bridge should be such as to cause the curve of equilibrium of the catenary curve to pass through the points above assumed as removed 6 inches away from the original curve. Mr. Phipps had calculated that a strain of about 130 tons must be deducted on the one side, and added to the other side, in order to reduce the motion to 6 inches, as above; and, in order to the due straining of the upper and lower booms to, say, 5 tons on the inch, the depth of the girders should be about 25 feet. Those girders would weigh about 130 tons, in lieu of the present girders computed at 60 tons. The sectional area of the chains would require an increase equal to 60 tons. Thus by the addition of about 130 tons of new material in the girders and 60 tons in the chains, the deflection of the chains might be reduced from 4 feet down to 6 inches. This appeared to be the theory of the matter; but it was quite likely that the above excess of weight might have to be somewhat increased when all the attachments of the tube or girders to the platform and other things were taken into the account. It would, of course, be necessary to provide for the lateral force of the wind against the girders; and it might also be remarked, as, perhaps, a practical objection of certain weight to the above system of giving rigidity to a suspension bridge, that the material in the upper and under booms of the girders would have the strain upon it changed from tension into compression every time the moving load passed over the bridge, and that the alteration of the versed sine of the catenary due to changes of temperature would, to some extent, increase the strain on one or other of the booms, beyond the strain at the mean temperature.

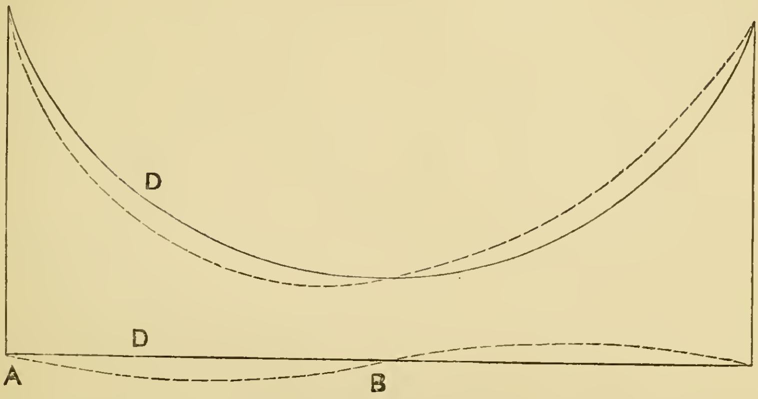
With regard to suspension bridges generally, it was evident that when consisting of one single span the quantity of material in the back-chains and anchorages became an important part of the whole. When there were several openings, such as in Mr. Vignoles bridge over the Dnieper at Kief, or in Mr. P. Barlow's Lambeth bridge, the proportion of the back-chain diminished; but when the chains were allowed to have motion on saddles on the piers, as in the former bridge, the difficulties of any arrangement for stiffening were much increased. The matter was, however, simplified when the chains were fixed to the tops of the piers, as in the Lambeth bridge; but it was questionable whether that plan did not produce a somewhat dangerous pull upon the piers, tending to overturn them, when the adjoining spans were unequally loaded. It was true the longitudinal girders, passing from abutment to abutment, and attached to the piers, seemed to give them support; but it appeared doubtful how far that action could be depended upon, in consequence of the effects of contraction and expansion on the girders and the chains.

Mr. J. M. Heppel said, much of what he had to adduce on the subject of stiffening suspension bridges, so as to make them applicable to railway purposes, had been anticipated by the remarks of the Astronomer Royal and Mr. Phipps, who had severally expressed almost identically the views he entertained.

Fig. 1 (p. 305), was intended to show the state of things when half the bridge was loaded with a rolling load and the other half was light; possibly that was a condition which produced the greatest disturbance of a chain with no assistance from stiffening. The point he had in view was to submit the kind of girder he would propose to make it capable of carrying a railway train. He assumed an admitted amount of deflection in the centre of the half-span. He put D to indicate the assumed deflection.

Calculating on that basis, to produce a given deflection there must be a certain ratio between the weight on one half and the weight on the other half, and that ratio would be solely dependent upon the geometrical form of the curve. Let this ratio be called M.

Fig. 1.



Let P (at present unknown) represent the distributed load on the half-span A B, which would produce the deflection D in the stiffening girder.

Let W represent the fixed load on each half-span, and R the rolling load on the half-span A B.

Then it was evident that the total load on the depressed half of the chain was $W + R - P$, and that on the elevated half was $W + P$; also, that these two loads must have the ratio M.

Consequently $$\frac{W + R - P}{W + P} = M.$$

Solving this equation with respect to P

$$P = \frac{R - (M - 1)\,W}{M + 1}$$

With respect to the ratio M, it was evident that it must be greater than 1, and that in all practical cases it could only exceed it by a small quantity; also, that the nearer M was to 1 the greater would P become, and consequently the stronger the girder must be made. A safe assumption was, therefore, to take $M = 1$, which reduced the above expresson to

$$P = \frac{R}{2}.$$

This, in words, meant that in order to stiffen a bridge to the required extent, the girder must be capable of supporting a distributed load equal to half the rolling load on half the span. Practically, the case was complicated with a vast number of considerations which the Engineer had to deal with, such as motion in the points of support, temperature, and consequent central deflection; but this was a distinct idea to begin with, and he did not think these circumstances would to any great extent modify the result arrived at. He had calculated what the effect of this would be if applied to the structure which had been described, and he found that a girder of about 130 tons weight would be required to be attached to the Clifton bridge in that way upon an assumed deflection of 4 inches. That girder, he calculated, ought to be 30 feet in depth, so that he could quite suppose the longitudinal girder actually used, being of much less depth, would not have any great effect in stiffening; but if the girders were 30 feet deep, and there was 130 tons of iron in each, he was convinced they would not be deflected more than to the extent named. Although he differed from Mr. Hawkshaw at all times with great deference, he could not admit that it would take as much metal to stiffen a suspension bridge as it would to construct an independent girder; because, in the case of the Clifton bridge weighing 1,000 tons, the additional weight of metal he proposed would be only 70 tons, and that would leave the total weight far below that of a girder for crossing so wide a span. He had no doubt that, with a weight of bridge very little in excess of 1,000 tons, a running load of $\frac{2}{3}$rds of a ton to a foot might be carried, but that would not be more than half what a railway bridge would require, to carry the heaviest kind of engines and rolling stock.

Capt. Tyler: You would not load a span of 600 feet with a ton per running foot?

Mr. Heppel said, in the designs for the proposed bridge over the Frith of Forth, $1\frac{1}{4}$ ton was taken for a span of 500 feet. Looking at it in that point of view, supposing a bridge had to be made on Mr. Barlow's plan, to carry $1\frac{1}{3}$ ton to the foot run, there would be just double what he had got, and that would require a bridge of the weight of 2,000 tons. He had gone somewhat into the question of large girders, and he thought it might be possible to make a girder of 700 feet span to carry $1\frac{1}{3}$ ton to the foot with a weight of 2,500 tons, but certainly not less. He was therefore disposed to think, notwithstanding what had been said, that there would be found to be an economy in the use of a stiffened suspension bridge, and that at any rate the matter was worthy the attention of Engineers; and more particularly so, if they should succeed in finding a material on which the Board of Trade would allow a greater strain than 5 tons to the square inch, as that would tend greatly to enhance the advantages of this system of construction.

Mr. Whiting ventured to join in this discussion, because of the suggestive remarks made by the Author, when directing attention to the advantages to be gained, first, by deepening continuous open girders over piers; and further, by utilizing more than at present the great tensile powers of steel. He would therefore briefly show that the compression in the upper chord might at once be converted into tension; for the strains at the junction of each diagonal with that chord could be resolved backwards, so as to accumulate and be borne by a chain increasing in strength towards the ends, fastened back or continuous, instead of being resolved towards the centre, where they at present accumulated, and were borne by compression bars increasing in weight where it was more prejudicial. This principle he begged to illustrate by a model, by which it was shown, that vertical pressure relieved the centre of the upper chord, which only required at that part to be of sufficient section to meet the greatest strain due to a partial passing load,—the strength of material being represented in the model as 3, 2, 1, commencing at the bank; but the lower chord was strongest at the centre, just as in ordinary Warren girders, and was not tied back or continuous over piers. It was further shown, that the form of the bridge was dependent on the relative lengths of the corresponding portions of the upper and lower chords, so that the principle was applicable to suspension chains, the upper chord being then in the form indicated in the suggestion of the Author of the Paper. He thought this method of making the chain part of a genuine girder gave several advantages. It might be easily constructed, and the chain and girder would work harmoniously in all temperatures; the strain due to changes of relative curvature of the upper and lower chord in the hanging girder, which had been alluded to by a former speaker, and had been investigated by Mr. Latham, would be here lessened by the freedom of the lower chord; the portions of a bridge thus constructed would not be cantilevers over piers; and the upper and lower chords might be steel in tension throughout.

The Astronomer Royal did not wish to avail himself of the permission of the President to enter upon the discussion of any controverted point; but allusion having been made to the effects of expansion by temperature, he would only point out, for the information of those who had not made the calculation for themselves, what was the amount of drop in any given suspension arch. The rule was a simple one. Take the radius of curvature of the chain, and increase it by one half, and the drop in the centre was the expansion due to a bar of metal whose length was equal to the radius of the curvature increased by one half. With regard to the effect of expansion on the construction which he had proposed, he had had it fully in his mind; and, taking the flexibility of the girder as he proposed, the effect of expansion by temperature was of no importance.

Mr. W. H. Barlow said, without going into all the points which had been brought forward during this discussion, he would, in as few words as possible, reply to one or two of the most prominent. In the first place, it was said that the angles at which the chains left the piers should be equal on each side. Mr. Vignoles did not say to what line he referred as a datum for the equal angles, but Mr. Barlow presumed that he spoke of a horizontal line. That was a condition which was only required to be fulfilled when the chain had to be of equal strength on each side of the pier, and when the saddle was free to move on a horizontal base. The angles might be varied, either by varying the strength of the chains on each side of the pier, or by giving an inclination to the bed of the saddle. He had been asked why he had given an inclination to the bed of the saddle? If it had not been so (inasmuch as the chains were of equal strength on each side), the chain on the land side must have been carried to an anchorage farther out than it was; but by giving this inclination to the saddle, the length of the land-chain was reduced to an extent which saved a considerable sum of money in the cost of the bridge. The inclination of the saddle was 1 in 20. The line of pressure, coming upon the pier at that inclination, fell only about 15 inches from the centre of the pier; and the pier itself weighed something like 4,000 tons, or about four times the weight which came upon it. The effect was to impose a certain amount of work on the pier; but it was an amount of work the pier was well able to sustain. The next point was that referred to by the Astronomer Royal, with regard to using a stiffening girder to equalize the action upon the three chains. Upon that he would say, the stiffening girders were not heavy ones; but the effect in equalizing the action on the three chains was very good indeed, and answered the intended purpose completely. The next point was as to the moving load of 70 lbs. to the square foot. It was true that it was possible to pack people so closely as to bring a pressure of 125 lbs. to the foot; but practically that load was never attained. It was a fixed load, and not a moving load, that had to be dealt with. On several occasions there had been on this bridge conditions of crowding, in which it was a work of labour and of time to walk from one end to the other. He had estimates taken of the number of people on these occasions, and it had never exceeded 5,000, which would not produce a greater pressure than 40 lbs. to the square foot. Moreover an increase in the load did not produce a corresponding increase in the strain upon the chain. With a load of 70 lbs. per foot, the strain was under 5 tons to the inch. If the load was doubled, the strain would only then be a little over 6 tons; and as every link had been tested to 10 tons to the inch, and was capable of bearing 20 tons to the inch, there was abundance of strength in the bridge. The same observation applied to the case in which there was a load on one half, and not on the other. Practically, that did not happen. Certainly Mr. Phipps' calculations of 4 feet never did arise either from a crowd or a gale of wind. With regard to the suggestion that land-ties should be applied to prevent oscillation, he would state that this bridge rose and fell as much as 12 inches from the effects of temperature. If land-ties had been made, they could only have been used with an arrangement to adjust them in summer and in winter. Any fixed ties which were made right for summer would be wrong in the winter; and he therefore thought it was best to do without them altogether. The only other point to which he thought it necessary to advert was as to the general question of the comparison between girders and suspension bridges. The former had found its advocates in Mr. Hawkshaw and Mr. Bramwell; but in what had taken place in this discussion, the predominance of opinion was, he thought, in favour of the views expressed in the Paper: in other words, that where very large spans were required, they could be crossed by stiffened suspension bridges more advantageously than by girders. If any one were called upon to build a bridge of 1,000 feet span for railway purposes, he thought no one would like to say that he could not do it; but he ventured to say that no one would undertake to do it by means of a girder.

February 26, 1867.

JOHN FOWLER, President,
in the Chair.

The discussion upon the Papers, No. 1,156, "Description of the Clifton Suspension Bridge," and No. 1,175, "On the Use of the Suspension Bridge with Stiffened Roadway, for Railway and other Bridges of great Span," was continued throughout the evening, to the exclusion of any other subject.

March 5, 1867.

JOHN FOWLER, President,
in the Chair.

The following Candidates were balloted for and duly elected:—Amias Charles Andros, Henry David Furness, Robert Edward Johnston, William Jarvis McAlpine, and Allan Wilson, as Members; Horace Bell, Francis Bramah Gilbertson, Spencer Herapath, George Houghton, Charles Harlowe Lowe, Major William Palliser, and Edward Pritchard, as Associates.

No. 1,160.—"On the Working of Steep Gradients and Sharp Curves on Railways."[1] By Captain Henry Whatley Tyler, Assoc. Inst. C. E.

The comparative terms, steep and sharp, have acquired at the present day a signification very different from what they conveyed to Engineers a few years since. The locomotive engine has been gradually trained and adapted to gradients of 1 in 100, 1 in 50, 1 in 25, and 1 in 12, combined with curves of from 30 chains down to 15, 10, 5, and even 2 chains radius; and during all this progress, the result of so much labour and ingenuity, the system of bite, or adhesion, by plain surfaces, has steadily triumphed as a means of converting steam-power into tractive force. The well-known rack-rail of Blenkinsop, as well as the Archimedian screw of Grassi, and the grooved wheels of other inventors, have all succumbed before it. The coefficient of adhesion was always in the first instance under-estimated. Legs or feet were most cleverly contrived to enable engines to walk, so to speak, before they could run; and the central rail system of Vignoles and Ericsson, patented so far back as 1830, was intended to provide extra adhesion on what are now considered moderate gradients, in place, apparently, of the rack-rail. The defects of the rack-rail appear to have been,—the risk of fracturing the teeth; the liability of the teeth to be choked with dirt, snow, or ice; the slip which resulted as the teeth began to wear, and the continued blows which they occasioned to the locomotive, causing it to be, in fact, always 'on the rack.' Grooved wheels afford obviously increased bite; but there must be, when they are used for locomotive purposes, continual abrasion from unequal travel of the surfaces in contact, with increased friction on curves, and some loss of power, in proportion to the increased bite obtained, from what may be termed back-adhesion. The screw of Signor Grassi—to work under his engine on a series of rollers along the permanent way—was reported upon by Captain Moorsom, in 1857. Captain Moorsom expected a load of 80 tons to be taken by its means, in place of 50 tons by "an ordinary bank-engine," up 1 in 20; but he stated that there would be three peculiar difficulties to contend with:—(1) The maintenance of exact action between the wheel and the screw. (2) The friction of the rollers. (3) Economy of maintenance of engine and road. The screw was to be 13 inches in diameter, winding round a shaft 7 inches in diameter, with a pitch of $12\frac{1}{2}$ inches, and 5 feet 4 inches long, grasping two rollers at a time on the permanent way. These rollers, $8\frac{1}{2}$ inches in diameter, were to be placed 3 feet 2 inches apart from centre to centre. A bevelled wheel on the driving-axle of the engine was to be connected with a crown wheel on the end of the screw shaft, and the proportions were to be such, that each revolution of the driving-wheel was to turn the screw twelve times. This system was ingenious, but wanted many of the advantages which will be hereafter referred to as arising out of the use of the central rail.

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[1] The discussion upon this Paper extended over portions of four evenings, but an abstract of the whole is given consecutively.

In this country the Lickey incline of 1 in 37, the incline of 1 in 30 on the Folkestone Harbour branch, the Oldham incline of 1 in 27, and the navigation incline of the Taff Vale Railway of 1 in 18, have, with others, been worked for a greater or less number of years with what may be called engines of ordinary construction. And it was stated in this Institution, in 1858, that an eight-wheel coupled engine, weighing 24 tons, was in the habit of taking its tender and a car-load of iron, together weighing 25 tons, over a gradient steeper than 1 in 10, at a speed of from 8 to 10 miles an hour.[1] This required, however, as will be observed, an adhesion of between one-fourth and one-fifth of the weight of the engine, which is more than could be relied upon, at all events in this climate. But in conveying heavy loads up gradients much less steep than those referred to, a want of extra adhesion has been seriously felt, and various expedients have been resorted to for obtaining it. The most comprehensive proposal with this view was that of M. Flachat, who, in a paper laid before the Society of Engineers in Paris, in 1859, desired, in constructing railways over the Alps, to utilize the adhesion, not only of all the wheels of the engine and tender, but also, by the use of additional cylinders, &c., to them, of all the vehicles composing a train. M. Flachat says, "La puissance motrice, au lieu d'être concentrée sur six roues, s'appliquerait ici à 32 ou 40 roues; elle sera donc

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[1] *Vide* Minutes of Proceedings Inst. C.E. Vol. xviii., pp. 53 and 54.

dans des conditions d'utilisation bien supérieures. Ce qui manquera aux unes, par une diminution éventuelle d'adhérence, sera reporté sur les autres, de telle sorte que cette puissance ne pourra s'annuler à la fois entièrement pour tout un train, comme cela a lieu par le patinage de la machine." Mr. Sturrock has acted in this country, *sed longo intervallo*, in the same direction, having contented himself with adding cylinders and the necessary apparatus to his tenders, which he has now for some time employed as assistant engines on certain parts of the Great Northern Railway. The engine and steam-tender together weigh, when fully loaded, between 60 and 70 tons.

M. Thouvenot, on the Continent, and Mr. Fairlie—who has published a most interesting pamphlet on the subject—in this country, employ two tank-engines in one, placed, as it were, back to back, and united as to their boilers and fire-boxes. They thus obtain double engines, which are intended to be worked by two men only, which have double power, intended to run either end foremost, and which are adapted for sharp curves. There being four cylinders, the wheels under each engine are coupled together, independently of those under the other engine. M. Thouvenot's engine weighs upwards of 80 tons, while Mr. Fairlie estimates his heavy goods engine at 60 tons, and states that it would draw (including its own weight) 170 tons, up a gradient of 1 in 12, at 10 miles an hour. But this is allowing between $\frac{1}{3}$rd and $\frac{1}{4}$th for adhesion; whereas if $\frac{1}{9}$th were allowed for adhesion, the same engine would only be able to take 72 tons altogether, or 12 tons besides its own weight, or $\frac{1}{5}$th of its own weight, up the same gradient.

In the ordinary system of obtaining adhesion by bearing-wheels only, whether of an engine and tender, or of a double engine, or of two engines coupled together, the weight of the motive power requires to be increased for a given amount of adhesion, in proportion to the load or to the steepness of the gradient. The limit of the gradient up which such an engine can take a load may roughly be defined by the coefficient allowed for adhesion; and it is necessary, of course, in practice, particularly on very steep gradients, to allow for, not an average, but nearly the lowest, rather than the best adhesion that can be obtained. If, for instance, $\frac{1}{10}$th be allowed, then 1 in 10 is (omitting friction) the gradient on which an engine may move itself, but on which no load can be taken. If $\frac{1}{12}$th be allowed, then 1 in 12 is the gradient of no load; and so, in all climates where from $\frac{1}{10}$th to $\frac{1}{12}$th only of adhesion can be relied on, no amount of increase in weight, or arrangement of the parts of an engine, will admit of trains being worked on such gradients. Mr. Fell, therefore, when brought face to face with the Mont Cenis, on which the adhesion may vary from $\frac{1}{6}$th to $\frac{1}{12}$th and on which gradients were required of 1 in 12, was obliged to adopt some other method than that of trusting to adhesion by bearing-wheels; and having a high summit to surmount, it was of great importance to him, with reference to cost of working, to save weight in the engine as well as in the train. By wisely adopting the principle of horizontal wheels and a central rail, he found the means of doubling the adhesion, at the same time that by the use of steel, he made the engine lighter than it would otherwise have been.

The central rail system was first patented, as already stated, for extra adhesion, by Mr. Vignoles, and Mr. Ericsson, on the 7th September, 1830; and next on the 15th October, 1840, by Mr. H. Pinkus in England. It was proposed by the Baron Séguier, in December 1843, to "L'Académie des Sciences," as a means of safety for general application, and patented by him three years later. It was again patented in England by Mr. Seller, under the name of A. V. Newton, on the 13th July, 1847; and lastly by Mr. Fell, who has, with the assistance and influence of Mr. Brassey, been the first to carry it into practice, in January and December, 1863.

It has been tried with two engines, and upon two experimental lines. The first experimental line, 800 yards long, containing 180 yards of straight line, on a gradient of 1 in 13·5, and 150 yards of curves, with radii of 2½ and 3½ chains, was on the Cromford and High Peak Railway, in Derbyshire. The second, a mile and a quarter long, on an average gradient of 1 in 13, containing, besides others, 480 yards of curves with radii varying from 4 to 2 chains, and terminating at an elevation of 5,815 English feet above the sea, was laid on the road over the Mont Cenis.

These lines were laid on a gauge of 1·10 mètre or (3 feet 7⅝ inches) with bearing rails of the ordinary description, but with a third rail laid on its side horizontally and centrally between them at an elevation (to its centre) of 7½ inches above them. On the Mont Cenis, the central and bearing rails were borrowed from the Victor Emmanuel Railway Company, and weighed about 75 lbs. to the lineal yard. The chairs which supported the middle rail were partly of wrought and partly of cast iron; they weighed 20 lbs. each at the joints, and 16 lbs. each in the intermediate spaces: they were placed 6 feet apart on the straight line, and from 2 to 3 feet apart on the curves; and they rested on longitudinal timbers, 8 inches deep by 12 inches wide, which were spiked to the transverse sleepers.

The first engine weighed 16 tons when loaded with coke and water. It has a heating surface of 420 square feet, and a grate area of 6 feet 6 inches. It is provided with four cylinders, two outside cylinders 11¾ inches in diameter, with a stroke of 18 inches, for working four coupled vertical wheels 2 feet 3 inches in diameter, with a wheel-base of 5 feet 3 inches; and two inside cylinders 11 inches in diameter, with a stroke of 10 inches, for working four horizontal coupled wheels 1 foot 4 inches in diameter, with a wheel-base of 1 foot 7 inches. It has a pressure of 16 tons on the horizontal wheels, or about the same weight as is carried by the vertical wheels. Guide-wheels have also been added to the trailing end, to act upon the middle rail.

The machinery of this engine is too much crowded together for convenience in re-adjustment or repair; its boiler power is not sufficient for working the traffic of the Mont Cenis; and the oil from its machinery falls upon the horizontal wheels, and deprives them, to some extent, of their power of adhesion; but it has answered its purpose in proving the principle which it was constructed to test, and has been, considering its novelty, a surprising success.

The second engine, constructed specially for the Mont Cenis, is partly of steel. Its net weight is now 14 tons, and its mean weight, when fully loaded with fuel and water, 17 tons, of which 2 tons 13 cwt. is for the machinery connected with the horizontal wheels. The boiler is 8 feet 4½ inches long, and 3 feet 2 inches in diameter, and contains one hundred and fifty-eight tubes of 1½ inch external diameter. The fire-box and tubes contain altogether 600 superficial feet of heating surface, and there are 10 feet of fire-grate area. There are only two cylinders, with a diameter of 15 inches and stroke of 16 inches, which work both the four coupled horizontal, and the four coupled vertical wheels, which are all 27 inches in diameter. The wheel base of the vertical wheels is 6 feet 10 inches, and that of the horizontal wheels is 2 feet 4 inches. The maximum pressure in the boiler is 120 lbs. This engine, without guide-wheels before or behind, travels with its longer (horizontal) wheel-base more steadily than No. 1; its machinery is more easily attended to, and the pressure upon its horizontal wheels can be regulated by the engine-driver at pleasure from the foot-plate. This pressure is applied through an iron shaft connected by means of right and left handed screws with a beam on each side of the middle rail, and these beams act upon volute springs which press the horizontal wheels against that rail. The pressure employed during the experiments was from 2½ to 3 tons on each horizontal wheel, or 10 tons altogether; but the pressure actually provided for, and which may, when necessary, be employed, is 6 tons upon each, or 24 tons upon the four horizontal wheels. The vertical wheels are worked indirectly by piston-rods from the front, and the horizontal wheels directly by piston-rods from the back of the cylinders.

The programme submitted to the French and Italian Governments, to serve as a basis for the locomotive trials to be made on the Mont Cenis, is as follows:—

MONT CENIS RAILWAY.

January, 1865.

Length of experimental line	2 kilomètres.
Elevation above the sea	1,773 mètres, or 5,815 English feet.
Mean gradient	1 in 13.
Maximum gradient	1 in 12.
Curves minimum	40 mètres radius.

In the demand for the concession presented to the French Government, the traffic of the Mont Cenis Railway was estimated at 2,500,000 francs per annum, consisting of 48,000 passengers, and 30,000 tons of goods; or 132 passengers and 82 tons of goods per day.

It is proposed to perform this service by three trains per day each way, in the manner described below:—

——	No. per Day.			No. per Annum.	
	Trains.	Passengers.	Goods.	Passengers.	Goods.
No. 1.—One train per day each way, carrying 40 passengers, viz., Pass. Trains. Pass. Days. 40 × 2 = 80 × 365.	2	80	..	29,200	
No. 2.—One train per day each way, carrying 26 passengers, viz., Pass. Trains. Pass. Days. 26 × 2 = 52 × 365	2	52	..	18,980	
also 20 tons goods, viz., Tons. Trains. Tons. Days. 20 × 2 = 40 × 365	..	..	40	..	14,600
No. 3.—One train per day each way, carrying 24 tons goods, viz., Tons. Trains. Tons. Days. 24 × 2 = 48 × 365	2	..	48	..	17,520
Total per day	6	132	88		
Total per annum				48,180	32,120

The weight and speed of the above trains will be as follows:—

No. 1.—Weight of train . . 16 tons.
Mean speed per hour . . . 12 kilomètres.
No. 2.—Weight of train . . 40 tons.
Mean speed per hour . . . 8 „
No. 3.—Weight of train . . 48 tons.
Mean speed per hour . . . 6 „

The results of the different experiments with these engines are all given in the following Tables, which show that considerably more could be performed than had been proposed in the programme handed to the French and Italian Governments.

(A.)

MONT CENIS RAILWAY.

Locomotive Trials for the French, Italian, and Russian Governments, and Others.

Summary of Distances and Speeds.

——		Time Employed.	Distance Run, 24 Tons load.	Steam pressure gained.	Time Employed.	Distance Run, 16 Tons load.	Steam pressure gained.
	Load.	h. m.	Kiloms.	lbs.	h. m.	Kiloms.	lbs.
July 19	3 runs 24 tons	1 00	10·920	75			
	2 ,, 16 ,,	..	..	..	0 30	7·280	70
,, 26	1 ,, 24 ,,	0 10	2·000	10			
	1 ,, 16 ,,	..	..	..	0 14	3·640	20
,, 27	2 ,, 24 ,,	0 36	7·280	45			
	1 ,, 24 ,,	0 17	1·720	10			
	2 ,, 16 ,,	..	..	..	0 27	7·280	47
,, 29	5 ,, 24 ,,	1 48	18·200	100			
,, 31	3 ,, 24 ,,	0 57	10·920	56			
	3 ,, 16 ,,	..	..	..	0 41	10·920	67
	23	4 48	51·040	296	1 52	29·120	204

Steam pressure gained 500 lbs, in 23 runs; or 21 lbs. on the average for each run.

(B.)

MONT CENIS RAILWAY.

Official Trials made before the French, Italian, and Russian Government Commissioners; also Mr. Brunlees and other Engineers, on the 19th, 26th, 27th, 28th, 29th, and 31st July, 1865.

Distance Run.	Average Speed in Ascending.	Load exclusive of Engine.
Kilomètres.	Kilomètres.	
51·040	10·704	24 Tons.
29·120	15·600	16 ,,
42·500	Speed not taken	Without load.
122·660		

(C.)

MONT CENIS RAILWAY.

LOCOMOTIVE TRIALS. CONSUMPTION OF FUEL.

Date.		Load 24 Tons.	Load 16 Tons.	Engine only.	Time Running.	Time Standing.	Engine under Steam.	Kilos. of Coke per Kilometres.	Total Consumption.	Total Consumption each Day.
	FOR ITALIAN COMMISSIONERS.	Kiloms.	Kiloms.	Kiloms.	h. m.	h. m.	h. m.	At	Kilos.	Kilos.
July 27	Distance run, 24 tons load	9·600	..	..	..	..	..	19	331	
	Ditto, 16 tons load . .	..	7·680	..	..	..	..	19		
	Ditto, without load . .	..	..	2·300	..	..	..	8	18	
	Time running . . .	..	..	..	1 30	..	..	..	..	
	Ditto standing . . .	..	..	..	..	4 55	..	45	221	
	Engine under steam . .	..	..	..	..	..	6 25	..	..	
	Consumption for the day	..	..	..	..	..	..	..	——	570
28	Distance run, engine light	..	..	27·800	..	..	..	8	222	
	Time running at 10 kilomètres per hour . .	..	..	..	2 50	..	..	..	..	
	Ditto standing . . .	..	..	..	..	4 25	..	45	198	
	Engine under steam . .	..	..	..	..	..	7 15	..	..	
	Consumption for the day	..	..	..	..	..	..	..	——	420
29	Distance run, 24 tons load	19·200	..	..	..	..	..	25	477	
	Ditto, without load . .	..	..	12·400	..	..	..	8	99	
	Time running, 24 tons .	..	..	..	1 48	..	..	..	..	
	Ditto, without load . .	..	..	..	1 15	..	..	..	..	
	Engine standing . .	..	..	..	..	4 02	..	45	180	
	Ditto under steam . .	..	..	..	..	..	7 05	..	..	
	Consumption for the day	..	..	..	..	..	..	..	——	756
	FOR MR. BRUNLEES.									
31	Distance run	11·520	11 520	..	2 03	1 27	3 30	..	445	445
	Total	..	..	..	..	..	..	..	..	2,191
	Distance run, 24 tons load	40·320	..	..	..	..	..	18·3	1,091	
	Ditto, 16 tons load . .	..	19·200	..	..	..	..	18·3		
	Ditto, without load . .	..	..	42·500	..	..	..	8	339	
	Time running . . .	..	..	..	9 26	..	..	..	..	
	Ditto standing . . .	..	..	..	..	14 49	..	45	664	
	Engine under steam . .	..	..	..	..	..	24 15	..	..	
	Extra fuel consumed for lighting Engine four times instead of twice, in running 102 kilomètres								97	2,191

		lbs.
Power employed for 24 tons load, 145 H.P. × 2 hrs. 24 mins. × 5 lbs. coke per hour		1,720
Power employed for 16 tons load, 172 H.P. × 56 mins. × 5 lbs. coke per H.P. per hour		805
		2,525 = 1,148 kilos.

Average consumption with above loads 18·3 kilos. per kilom. run.
,, ,, engine without load 8 kilos. per kilom. run.

Consumption per horse power 5 lbs.
,, ,, engine standing and steam blowing off 4½ kilos. per sup. foot grate area per hour.

Consumption of Water.

1 kilo. coke evaporated 8·76 kilos. water; 1,446 kilos. coke having evaporated 12,669 kilos. water.

Production of Steam.

With the 24 ton load the pressure in the boiler increased on an average 20 lbs. each run; and, with the 16 ton load, 25 lbs. each run.

It may be worthy of notice, that while a greater horse power was developed, and the consumption of steam was proportionably greater with the 16-ton than with the 24-ton load, the pressure in the boiler increased in a greater ratio also.

(D.)

TRIALS made on 29th and 30th November, before Messrs. BRASSEY, BLOUNT, BUDDICOM, and FOLSH.

The maximum speed with a 16 ton load taken over the whole 2 kiloms., was 18 kiloms. per hour. This gave one horse power to each 3 sup. feet of heating surface as a maximum. The steam pressure increased 20 to 30 lbs. With a load of 24 tons the maximum speed was 12 kiloms. per hour. The engine ascended a gradient of 1 in 12 without a load, with only 40 lbs. of pressure in the boiler.

Trial of Breaks.

Ordinary and centre rail breaks combined. With a gross load of 41 tons, descending a gradient of 1 in 12, at a speed of about 6 kilomètres per hour, the train was stopped within 20 mètres.

With a gross load of 33 tons, descending, under similar circumstances, at a speed of about 12 kilomètres per hour, the train was stopped on a given signal in 20 mètres.

(E.)

The proposed working programme for the Mont Cenis Railway is as follows:—

GOODS TRAINS.

	Tons.
Weight of engine . . .	20
,, train	40
Average net weight of goods carried .	25

	Francs.		Francs.
80 kilomètres at	1·55	=	124·00
25 tons . . at	5·00	=	125·00

50 miles at 2*s.* = £5.

or, 25 tons at 4*s.* = £5.

Giving nearly one penny per ton per mile for locomotive power, including fuel, grease, wages, and maintenance.

PASSENGER TRAINS.

	Tons.
Weight of engine . . .	18
,, train	24
Total	42

Locomotive Power.

	Franc.		Francs.	
80 kilomètres at	1·22	=	96·00	= cost for one journey.

$$\frac{\text{96 francs}}{\text{60 passengers}} = 1\cdot 60 \text{ franc per passenger.}$$

$$\frac{\text{1·60 franc}}{\text{80 kilomètres}} = 0\cdot 02 \text{ franc per passenger per kilomètre for locomotive power.}$$

Giving 1*s.* 3*d.* per passenger per journey.

The Author himself observed, what is quoted in one of the above experiments, that No. 2 Engine was just able to move up a gradient of 1 in $12\frac{1}{2}$ with 40 lbs. of steam-pressure. This engine developed 12 horse-power per ton of its own weight; but it is believed that by some alterations in the boiler, as well as in other parts of the engine, in which steel may be substituted for cast iron, something like 15 horse-power per ton may ultimately be obtained, as against 20 horse-power per ton which is afforded by steam fire-engines.

It was remarked during the later trials, that the engine and train gained speed on the sharpest curves. This effect, so contrary to general practice, was produced, partly by the action of the horizontal guide-wheels, which kept the engine and the wagons in their proper positions with respect to the rails, and partly to the fact that the gradients on the curves had been slightly eased, while the gradients on the straighter portions had been made proportionally steeper, with the intention of as nearly as possible balancing the resistances. There is the less practical difficulty in carrying out this advantageous arrangement upon very sharp curves, because such cannot of course be of great length.

The advantages of this system for mountain passes are very great. The middle rail, besides being of service in the ascent, affords the means of applying pressure-breaks, acting with any amount of force, to any number of vehicles in the descent, and thus renders the descent safe, and supplies a remedy against bad consequences from a fracture of the couplings. It also prevents the engine, or any vehicles of the train that are supplied with guide-wheels, from leaving the line, from a defect in the permanent way or rolling stock. The force of the wind is at times so great on the Mont Cenis that it would hardly be safe, if only on that account, to take trains over it without the protection thus afforded.

The Mont Cenis Railway is, however, expected to be at work from Susa to Lanslebourg or Modane in about three months, and in from five to six months throughout its whole length of 48 miles from Susa to St. Michel de Maurienne, and there will then be an opportunity of experiments over longer distances.

There is another system for working steep inclines which has found support in Italy, and which it will be proper to describe here —that of Signor Agudio. In this system two stationary engines are employed, one at the summit and the other at the bottom of an inclined plane. They have the same power, and they act upon the same double endless rope, which is kept stretched by a tension wagon hanging upon it at each extremity. This rope runs between the rails, and over two systems of wheels worked by the stationary engines, from which it receives its movement by friction. It does not act directly upon the train, but is connected with an engine which may be called the locomotive of the system, and has received from the inventor the name of "Locomoteur funiculaire." This is a vehicle 22 feet long, supported on a bogie-frame at each end, and carrying a system of drums and wheels, by the action of which the required motive power is obtained indirectly from the moving rope. The two portions of rope act upon separate wheels, the wheels set the drums in motion, and the drums climb a heavy stationary iron cable, firmly fixed at the summit and weighted below, which is called the "cable of adhesion." The ascending portion of the moving rope has two turns round two wheels on the left side, and the descending portion two turns round two wheels on the right side of the locomotive, the front wheels in each case remaining free, and being used for conducting the rope only, while the hind wheels transmit to the drum the moving power of the rope. The ascending rope acts through its (left) hind wheel upon a middle friction drum, which is compressed between the outer main drums by the force of the rope passing over them, and which thus turns the main drums on each side of it by adhesion. The descending rope acts through its (right) hind wheel upon a pinion and a rack in the inner circumference of the hinder drum. The drums being set in motion, the locomotive is moved by their friction upon the cable of adhesion which has two turns round them.

The middle drum which transmits the force of the ascending portion of the rope to the main drums, and the rack and pinion which transmit the force of the descending portion to the hinder drum, are so proportioned that the rope moves at two and a quarter times the speed of the locomotive; and as the two portions of the rope work equally, the strain on the rope is, excluding friction in both cases, $\frac{1}{2{\cdot}25} \times \frac{1}{2} = \frac{1}{4{\cdot}5}$, or $\frac{2}{9}$ths of what it would be in the case of a single rope acting by direct tension in the ordinary way. The moving rope of the system may therefore be proportionately diminished in strength and weight, or a greater length of inclined plane may be worked than under the ordinary system, with greater safety, though at the expense of a certain amount of complication.

The hinder rope-wheels of the locomotive are provided with gearing which will admit of their running free, or with their shafts, at pleasure. Before starting they are allowed to run free, and when a certain velocity has been attained, that which is connected with the friction drum is suddenly put into gear, and the excess of velocity is utilized in overcoming the inertia of the train and the apparatus. In ascending, the train may at any moment be stopped by putting these wheels out of gear and applying the break. There is a break attached to one of the drums and a sledge break on the rails, which may be used in the descent, when the moving ropes are stationary and the rope-wheels run freely on their shafts.

Some experiments which were tried with this system on the Dusino incline between Turin and Genoa in August, 1863, appear to have given great satisfaction to the Commissioners of the Royal Institute of Lombardy. The incline was 2,400 mètres ($1\frac{1}{2}$ mile) long, on variable gradients of 1 in 26 to 1 in 31, and the sharpest curve had a radius of 350 mètres, or $17\frac{1}{3}$ chains. The total passive resistance for a train of 120 tons on this incline was estimated at 44 per cent.; and it was calculated by the inventor that he would be able to save 605,245 francs out of 776,228 francs annually in the working of the Giovi incline. He proposed, however, to employ water-power for his stationary engines.

The useful effect of the system is calculated at 57·7 per cent. by the inventor as the result of the experiments on a length of $1\frac{1}{2}$ mile; at 50 per cent. for a length of 5 kilomètres (3 miles) by the Italian Commissioners who reported on the Mont Cenis Railway; and at 47 per cent. by M. Desbrière[1] for a length of 6 kilomètres ($3\frac{3}{4}$ miles). M. Desbrière represents it perhaps rather too favourably in allowing no diminution of useful effect as the gradients become steeper. It is true that the friction of the parts would remain nearly the same, but the weight of the locomotive being constant, it would bear a greater proportion to the trains as the gradients became steeper, and to that extent diminish the useful effect.

It is a question, whether, in the practical application of the system, the toothed wheels through which the force of the descending portions of the rope is applied, should not be replaced by wheels acting by adhesion; and it is a still more important question whether, supposing the principle of the moving ropes to be applied, a middle rail ought not to be employed in the place of the "cable of adhesion," as has been, indeed, already proposed by the inventor. Horizontal pressure-wheels in the locomotive might be made to act with any required amount of pressure upon a middle rail, in place of the drums acting upon the cable of adhesion, and many important advantages would thus be obtained. 1. The weight of the locomotive might be reduced. 2. Much sharper curves might be employed. 3. A greater degree of safety would be attainable in the important items of (*a*) preventing, by the use of the horizontal wheels, all risk to the locomotive or any of the vehicles behind it from their leaving the rails, (*b*) affording a means of applying a break, by pressure upon the middle rail, which would admit of a train being stopped without difficulty upon any gradient.

A modified Agudio system—with the double-wire rope and a middle rail—worked by stationary engines, might probably in some cases be adopted with advantage for passenger traffic on gradients steeper than 1 in 10 or 1 in 12. The defects of the system—that it can only be worked for lengths of 4 to 5 miles, and that it does not admit of the use of very sharp curves – would be insignificant if gradients of 1 in 4 or 1 in 5 were employed in suitable localities. At high elevations, however, where the ropes would be liable to be covered with snow and ice, the system would be inapplicable, at all events excepting under complete cover; and steam-power would be required, inasmuch as water-power would not be available in seasons of low temperature.

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[1] In a valuable pamphlet, entitled "Etudes sur la Locomotion au moyen du Rail Central," Extrait des Mémoires de la Société des Ingénieurs Civils, 1865.

It does not appear that this system, or any other yet developed, can compete with the central rail system for general traffic on very steep gradients up to 1 in 10 or 1 in 12; and the principal questions that remain to be considered are, the relative economy of summit lines with steeper gradients, and tunnel lines with less steep gradients, and the limit from which the central rail may be profitably employed. The best comparison that can at present be made in regard to the former point is between the Mont Cenis Railway and the Grand Alpine Tunnel.

The Italian Commissioners who reported on the experiments on the Mont Cenis, themselves admit a saving of 84,000,000fr. out of 123,000,000fr. in favour of an improved and permanent summit line, as compared with the tunnel line which the Italian Government is engaged in constructing to connect Modane with Susa. They arrive at this result by deducting 16,600,000fr. as the cost of an improved summit line, and 22,400,000fr. at which they estimate the capitalized difference of working expenses (over a super-elevation of 2,500 feet) from the estimated amount, as above, of 123,000,000fr. for the tunnel line. But Mr. Fell points out that they have omitted 10,000,000fr. for the portion of their own line between Modane and St. Michel: they have inadvertently charged 1,000,000fr. for extra rolling stock for the summit line twice over; and they have overcharged the capitalized extra working expenses by 10,000,000fr. on the one hand, at the same time that they have omitted 800,000fr.—the cost of the extra portion of the tunnel line from Susa to Bussolino—on the other hand. The Italian estimate thus modified shows a saving in favour of the summit line of 104,800,000fr. out of 133,800,000fr., or, in other words, places the total cost of constructing and permanently working this particular summit line at less than one-fourth of the tunnel line.

The Italian Commissioners who reported to their Government upon the best mode of crossing the Swiss Alps, took considerable pains, also, to calculate the relative cost of conveying passengers and goods by tunnel lines or summit lines such as that over the Mont Cenis. Taking into account the total capital to be expended, and the cost of working in each case, they came to the conclusion that the cost of the tunnel line would be: for goods, 28 centimes per ton per kilomètre, and for passengers, 17 centimes each per kilomètre; as against on the summit line: for goods, 10 centimes per ton per kilomètre, and for passengers, 6 centimes each per kilomètre, showing that there would be a reduction of total cost amounting to about 64 per cent. in favour of the summit line, with a loss of time, for passengers, of thirty-eight minutes upon 48 miles against the summit line, in the passage of the Mont Cenis.

The particular gradients on which the central rail may properly be applied must, of course, vary with the coefficient of adhesion and other local circumstances, and be left in each case to the discretion of the Engineer.

Neglecting the questions of speed and steam-power, and assuming $\frac{1}{10}$th as the coefficient of adhesion, then the proportions of net load that could be taken up the following gradients by two engines, each of 20 tons, one of ordinary construction, and the other with horizontal wheels and a supplementary adhesion of $1\frac{1}{2}$, would be respectively :—

for 1 in 20	20 tons net load for	ordinary engine	or 1 to 4
	80 ,, ,,	central rail engine	
,, 1 in 16	12 ,, ,,	ordinary engine	or 1 to 5
	60 ,, ,,	central rail engine	
,, 1 in 12	4 ,, ,,	ordinary engine	or 1 to 10
	40 ,, ,,	central rail engine	

Similarly for an adhesion of $\frac{1}{7}$:—

for 1 in 20	39 ,, ,,	ordinary engine	or 1 to $3\frac{1}{4}$
	129 ,, ,,	central rail engine	
,, 1 in 16	27 ,, ,,	ordinary engine	or 1 to $3\frac{1}{2}$
	99 ,, ,,	central rail engine	
,, 1 in 12	15 ,, ,,	ordinary engine	or 1 to $4\frac{1}{2}$
	69 ,, ,,	central rail engine	

But these advantages would, of course, only be available as long as the adhesion was insufficient in an ordinary engine for the steam power, and would disappear in such a case as that which—though it can hardly be credited—was reported from the Alleghanny Mountains in America, in the Paper already referred to, where, on a gradient steeper than 1 in 10, an ordinary engine (with an adhesion apparently of $\frac{1}{4}$) is stated to have worked with a load. The following Table, from the Paper of M. Desbrière, before alluded to, gives the result of his calculations.

Table of minimum weight to be allowed, with an adhesion of $\frac{1}{10}$th to

(1.) An ordinary locomotive engine with wheels all coupled.

(2.) A central-rail engine, with 1·5 of supplementary adhesion, to draw a net load of 100 tons on different gradients, compared with the weight which would result, for each system, with heating surface corresponding to various speeds:—

Gradients.	Weight necessary for adhesion of Locomotive Engines.		Speed in Kilomètre per Hour.	Total Force.		Weight required for Steam Power.	
	Ordinary.	Central Rail.		Ordinary Engine.	Central Rail Engine.	Ordinary Engine, System Pétiet, 120 kilos. per H.P.	Central Rail Engine, 112 kilos. H.P.
Millims.	kilos.	kilos.	kiloms.	H.P.	H.P.	kilos.	kilos.
0	5·000	2·000	72	146·66	146·66	16·352	16·352
10	16·440	6·250	30	203·33	190·00	24·400	21·280
20	31·250	10·860	20	254·80	217·77	30·576	24·390
30	50·000	15·900	15	305·50	243·33	36·660	27·252
40	75·000	21·430	12	366·60	257·30	43·992	28·617
50	110·000	27·500	10	448·10	280·00	53·772	31·360
60	162·500	34·730	10	659·40	346·86	78·128	38·852
70	250·000	41·660	10	1,014·75	422·50	121·680	47·320
80	425·000	50·000	10	1,725·00	482·80	207·000	54·073
90	950·000	59·440	.	..	..	..	..
100	..	70·000	..	..	..	..	..

Precise calculation is, however, of limited value, when the coefficient of adhesion, the principal element, is so very variable. But no English Engineer would probably contemplate working any considerable length of railway permanently on a steeper gradient than about 1 in 25 without the margin for adhesion afforded by, and the additional safety of, the central rail; and it might, no doubt, be frequently used with advantage on gradients less steep than 1 in 25. A country which requires very steep gradients demands also, in most cases, very sharp curves; and the central rail contributes to safety as much in respect to the latter as to the former. It also contributes in an important degree to economy, by diminution of friction in passing round very sharp curves, by which loss of power and wear and tear are equally avoided. And it may be added, in conclusion, as a result of experience, that the bearing-wheels of the engine left the bearing-rails once during construction, and once before the Italian Commission, on the Mont Cenis Experimental Railway, and were brought back to them on both occasions by the guiding power of the central rail.

Captain TYLER thanked the Meeting, in the first place, for the way in which his Paper had been received; and Mr. Fell, in the next, for a great deal of the information contained in it. Since it was written, Mr. Fell had made some improvements in the engine, which he would be able to explain in person. He had also to thank Mr. Brunlees, the Engineer of the Mont Cenis Railway, for having prepared the large plan and section of that railway, with the section showing the central rail, and its attachment between the bearing rails.

There was one interesting question to which he had only briefly alluded, and that was with regard to the engines for working both severe gradients and sharp curves, which were advocated by M. Thouvenot, in France, and Mr. Fairlie, in this country. The latter adopted the principle of attaching the boilers of two engines together, firebox to firebox, and carrying them upon two bogie frames, the wheels under these frames being respectively coupled together and worked by separate cylinders. The former, by using peculiar cranks and coupling chains, proposed to transmit the power obtained by his double engines to the wheels of the carriages. There was no doubt much in favour of Mr. Fairlie's form of locomotive for certain purposes. Captain Tyler considered there was frequently a certain amount of extra risk in the employment of tank engines. He had always been of opinion that they were not calculated to run safely at high speeds, especially with passenger trains, and he thought such engines might properly be superseded, at all events for passenger traffic, by engines of a construction somewhat similar to that advocated by Mr. Fairlie. At the same time, tank engines were found to be convenient for the mineral traffic of short lines, for heavy gradients, and for frequent stoppages. In working steep inclines. one tank engine was often placed in front, and another at the tail of the train; and this was in one respect a safe mode of working, inasmuch as if a coupling gave way there was an engine as well as a break-van behind to prevent any of the wagons from running back.

He had omitted all notice of the Pneumatic system, because he did not think it applicable to the working of very steep gradients. It might perhaps be usefully employed in tunnel lines for metropolitan passenger traffic, on account of the absence of steam and smoke; and he thought it was a pity it had not been tried in practice for that purpose.

In contrast to the working by the central rail system on the Mont Cenis, he would refer to some practical results of the working of a steep gradient on a railway which he had recently visited, viz., the Navigation incline of the Taff Vale Railway, near the Aberdare Junction, 16 miles from Cardiff. He was accompanied by the Engineer of the line, Mr. George Fisher, who had furnished him with the following particulars of the incline, and of the engines with which it was worked. The gradients were, in ascending, successively, 1 in 28, 1 in 21·80, 1 in 20·74, for shorter distances, and then 1 in 17·80 for 420 yards. The average of the whole was about 1 in 20 for half a mile. The system of traction by rope, on which this portion of line was formerly worked, was abandoned two years ago, and since that time locomotives had been employed. The expense for six months was £900 with the locomotives against £700 with the rope and stationary engine. The tank engines, specially employed for working the traffic up this incline, weighed in working order 36 tons. They had six wheels, all coupled, 4 feet in diameter. The diameter of the cylinder was 16 inches, with a stroke of 24 inches. The pressure of steam in the boiler was 130 lbs. The maximum load was 45 tons, and the regulated load 25 tons: the former giving $\frac{1}{8}$th and the latter $\frac{1}{10}$th as the coefficient of adhesion. It was interesting to compare these results with the working of the engines on the central rail system. On the Navigation incline, the engine weighed 36 tons, taking a regulated load of 25 tons up an incline of 1 in 20; whereas, on the central rail system, an engine weighing only 20 tons could take a load of 40 tons up an incline of 1 in 12. In fact, the programme of working the Mont Cenis Railway provided for an average gross load, exclusive of the engines, of 40 tons for goods trains, and 25 tons for passenger trains; and the cost of locomotive power was found to be nearly a penny per ton per mile for goods, including fuel, grease, wages and maintenance. Similarly, for passenger traffic, the cost for locomotive power for one journey from St. Michel to Susa was estimated at 96 francs, which gave a little more than a farthing (1·2 farthing) per mile for each passenger.

Mr. Fell said he was glad to have the opportunity of describing some details of the engine, and the improvements to which Captain Tyler had referred. When this project of crossing Mont Cenis by a railway was first laid before the Italian Government, it was proposed to divide the line into two distinct sections, one section being from St. Michel to Lanslebourg, and the other from Lanslebourg to Susa. It was proposed to work the first portion of the line by an engine with three or four cylinders, one pair driving the four outside wheels, and the other pair, or one cylinder, driving the four horizontal wheels, so that the relations of the interior and exterior wheels might be distinct. By that means wheels of comparatively large diameter could be used for the ordinary driving wheels, and small and more powerful ones for the climbing wheels. With the same speed of piston a higher velocity could then be obtained, and time saved in the journey, or more time could be allowed for ascending the heavier gradients. It would be seen that, with an engine of so novel a kind, the form best adapted for such a line as this could only be determined after a long series of experiments. There were many requirements which could only be ascertained by actual trial. The Directors of the railway had determined to adopt one type of engine only for working both sections of the line, and the second engine having only one pair of cylinders, had consequently been modified with that view. Originally there was no connection between the two pairs of wheels working on opposite sides of the centre rail. Some method of connecting the two was, however, found to be necessary; and on the experimental engine this was produced by the introduction of a series of toothed wheels. The adhesion on the centre rail gave continuity of movement, and when one crank was at mid-stroke it helped the other over the dead point. When running off the centre rail this was not the case, on account of the play between the toothed wheels; consequently the crank on the midstroke passed that point more rapidly than the one on the dead point, thereby producing a shock upon the engine. This shock would not take place if the inside wheels were driven by an independent pair of cylinders; but the pistons, being tied together where only one pair of cylinders was used by the outside gearing, moved in a way which produced that injurious effect. It thus became necessary to substitute some other system in place of the toothed wheels, which were weak and liable to rupture. Various plans had been proposed, out of which two were selected by Mr. Brunlees for experiment. One was the system of levers, jointed to the outside frames of the engine, connected in the centre by a slide block and with two offset cranks by connecting rods. The offset cranks were respectively placed at an angle of 45° with the driving cranks, which gave a leverage of 6½ inches, as compared with 8 inches the leverage at midstroke or 90°, which was enough to carry the cranks over the dead points. The relative position of the four cranks being such, that when one driving crank was at midstroke, and the other on the dead point, the offset cranks were each at an angle of 45° with the driving cranks, and acting with the above-named leverage of 6½ inches. This arrangement had been tried on the Mont Cenis, and had proved successful in carrying the cranks over the dead points without the shock, and without the danger of fracture, which was unavoidable in the case of the toothed wheels: and it was therefore the plan of engine now adopted for the Mont Cenis Railway. Trials had been made of another plan, which had been devised for the same object, but which had the disadvantage of increased weight. The adaptation of the levers added only about 6 cwt. to the weight of the engine, while the other plan, which consisted of a combination of toothed wheels, combined with frictional gearing, added 9 cwt. to its weight, having besides the disadvantage of greater risk of fracture.

Some other alterations had also been made. The rocking shafts were originally placed in front of the cylinders, in which position they worked very well, but to examine the cylinders and take off the covers, it was necessary to take down the slide bars; it had therefore been decided to move the rocking shafts to a position behind the cylinders. At present they were hung immediately under the boiler.

There was another point of importance, viz., the general insecurity and instability of tank engines at high speed, which had been alluded to by Captain Tyler. It was necessary to consider this point, particularly in an engine with an overhang which exceeded the length of the wheel base. There appeared to be a risk attending such an arrangement, and the question having been considered, it had been decided to adopt a third pair of wheels with a lateral movement, by which it was believed sufficient stability would be obtained to make the engine perfectly safe, but with the disadvantage that 12 cwt. would be added to the weight of the engine, and 2 tons of the adhesion obtained from its weight would be sacrificed.

Another point of great importance was the action of the breaks in descending the inclines. As had been stated, a great amount of break power was necessary for these trains, and this could be obtained by the centre rail system, which might in fact be considered almost unlimited. It had been decided to have centre rail breaks as well as the ordinary breaks, attached not only to the engine, but to each carriage and each wagon of the train; and probably the action of the combined breaks on a single carriage would be sufficient, or could be made so, to stop the whole train on any incline on the line. Then came the question of wear and tear upon the rails. The breaks now proposed were sledge breaks, forced on to the rails by a transverse screw or lever, which would produce a certain amount of wear. Another plan was under consideration for applying the breaks to the horizontal wheels, by which method it was believed the wear upon the centre rail would not be greater than upon the bearing rails. With regard to the comparative economy of the two systems of working inclines, some persons were under the impression that the cost of traction by the centre rail system, when a given summit had to be attained, was greater than under the ordinary system, but it was not so. Supposing an elevation of 2,000 feet had to be attained, it could be shown that in the case of a gradient of 1 in 30, without the centre rail, and in another case, with a gradient of 1 in 12 with the centre rail, the locomotive power, though considerably greater in the latter, being required for a proportionally diminished distance, the expense of working would not be increased, and time would be saved in the journey.

There were two modes of dispensing with the centre rail at level crossings: one in which it disappeared altogether; the other a simple arrangement by which it took the position of the ordinary rails, and laid flat.

Mr. Alexander remarked that the expedients resorted to in the construction of the engines had already been fully explained. In the plan of toothed gearing, first proposed by him for connecting the inner axles together, he found, as might be expected, a difficulty in getting the inner system to revolve. No apparatus was applied without first giving the opportunity to revolve. The engine was, in fact, tried without spur-wheels in the first instance; but the axles refused to go round, and always came back again. He then anticipated it would be a more simple affair to connect the wheels than it proved to be. With respect to the introduction of the toothed gearing, it was open to the objection that it made a great clatter when the engine worked off the mid-rail. He did not know that that was a fatal objection; but it was at best a rough contrivance, which could hardly be tolerated in these days of mechanical perfection; and there was an objection to the clattering noise, because if the driver were accustomed to so much noise, he would be apt to disregard the noise occasioned by anything going wrong with the machinery.

Many appliances had been suggested to connect these wheels, but serious difficulties arose from the fact that it was necessary to have variable distances between the axles. There was a continual and progressive wear going on at the tires. Also in running upon the mid-rail, which was tapered off to a fine point, the wheels receded from each other, and there was an immediate change of an inch or so in the distance of the centres. It was not like the ordinary coupling-rod system, in which the wheels revolved at the same distances and in the same direction. The system shown in the model explained by Mr. Fell seemed to answer well, and on being tested proved efficient; but Mr. Alexander believed it could be simplified, the same principle being retained. As this, however, had been so far actually tested, and its capabilities were known, and as it was difficult to predict what would be the results with an untried arrangement, it was proposed, in the engines now building, to use the method without alteration.

With reference to the amount of power developed by these engines in proportion to their weight, he, having designed them, was sorry to say he could not agree with Captain Tyler as to the advantage to be derived from the use of steel. He was anxious to use steel, as it was important that these engines should be as light as was consistent with safety. He had communicated with many Engineers in order to ascertain what had been done in this direction; but he did not find that in any articles subject to transverse torsional strain, with sudden shocks, like that upon axles, there had been any reduction of weight in consequence of the use of steel; in fact there was sometimes an excess of weight, and he had been reluctantly compelled to abandon the use of steel under such circumstances. His reason for that was that its extensibility was less than that of iron, its life was sooner exhausted, and it was more liable to give way under shocks. These engines were being made in France, where the use of steel was not carried so far as in this country, as there was not the same confidence in it; and there were many cases in which iron was used where, in this country, steel would have been employed.

Mr. Mendes Cohen, of New York, would give the results of his experience in working the Baltimore and Ohio Railway, through Virginia. The gradients on that line were generally heavy. On the mountain division, 60 miles in length, there were 37 miles varying but slightly from 1 in 45, at which maximum there were 17 miles in one continuous gradient. The goods traffic was worked by engines weighing, exclusive of tender, about 27 tons, on eight connected chilled wheels 43 inches in diameter, with cylinders of 19 inches diameter and 22 inches stroke, hauling nine cars weighing about 135 tons. In addition to these there were other and much heavier gradients of a temporary character, adapted for the purpose of working over the tunnel ridges during the progress of the construction of the tunnels, to continue the line without waiting for the completion of the tunnel-work. In the first instance the gradient adopted was 1 in 10 as a maximum, and it was not intended to work this temporary line with locomotives. The line was built with a view of hauling car-loads of iron across the mountain by horse-power, and continuing the construction of the works on the other side. However, when the line was laid, it was determined to try the working with locomotives. The engines just described were tried on this gradient and readily took up a load of one car weighing about 14 tons. Under favourable circumstances they could take up two, but one was the usual load. He had ridden over that line on the engines but otherwise had not much experience in the working. As the construction of the line progressed westward, there was another tunnel on which he had been engaged as an assistant in the engineering department in the year 1852. Owing to delay in the tunnel, the Chief Engineer, Mr. Benjamin H. Latrobe, directed the construction of a temporary road across the hill. It was a work of some difficulty, as the slopes of the hill were very abrupt, and he was instructed by his chief to see what he could do in the way of bringing the line down from the summit to the foot of the hill, with a limit of gradient of 1 in 16, and reversing the direction as often as was necessary. The descent of the summit on the western side was made with five reversals, the narrowness of the ravines and the general shape of the ground not always affording room for curves of even the minimum radius of 300 feet. The reversal was effected by what was termed a Y, from its resemblance in plan to that letter. With this arrangement the slope of the hill was descended on a gradient of 1 in 16 between one pair of Y's, and 1 in 20 between the next pair, and so on alternately, the object of this being to secure the proper protection of the fire-box sheets, which might have been exposed had the rear of the engine been uppermost on the heavier gradient. The line was worked with five reversals on one side and two on the other for a distance of 2½ miles on both sides of the hill, over which the engines carried three cars of 15 tons each. The load was never increased beyond that, because the shape of the ground did not admit of getting longer trains upon any of the Y's. The traffic was carried on for five or six months, till the tunnel was completed. At a later period it was necessary, for a second time, to use the temporary track over the tunnel where the gradient of 1 in 10 had first been tried. On this occasion the gradient was reduced to 1 in 20, or equal to 260 feet per mile, and this Mr. Cohen himself worked with the same engines, which carried up from 65 to 70 tons load. In fact the whole traffic—mails, passengers, and goods—of one of the great American through lines was carried on by this way for many months during the arching of the tunnel. There was no difficulty in ascending the hill; the only difficulty lay in the descent. In effecting this, besides the application of breaks to all the wheels of the train, the engine was reversed and allowed to descend without using steam, thus pumping air through the cylinders. The accumulating pressure against the pistons was relieved by valves placed on the steam-chests, and regulated from the foot-board. The engines were kept, on the heaviest portion of the gradient, on the lower side of the train, to guard against the breaking of the couplings, as well as with reference to the fire-boxes. There were some instances of the trains breaking away, from the failure of the breaks, and some little damage had been done; but no case of serious accident occurred during the whole time.

Sir Cusack Roney said he could throw no light on the subject with respect to the engineering details connected with the crossing of mountain passes by railways; but as the nature of his engagements for the last two or three years had taken him a great deal to Switzerland, and as he had been in constant communication with persons who took an interest in these matters, especially with reference to the passage of the Alps, he might be able to adduce a few facts of interest.

The trade between Great Britain and the East was enormous; and it would be seen, by the table of exports and imports, that both the exports to the Eastern seas, and the imports from them, amounted to about one-fourth of the gross trade of the United Kingdom. The Board of Trade returns showed that the value of the export trade for the year 1866, was 188,000,000*l.*, or an increase of about 23,000,000*l.* compared with 1865. No doubt the same proportion of increase existed with reference to the trade from the Eastern seas to Great Britain. It was, therefore, of the highest importance to this country to have the most rapid modes of conveyance for postal communication with those seas; and no doubt the passage of the Alps by a railway would effect a complete revolution in the communication between Great Britain and the East. It would be seen by the table showing the distances in English miles between London and Alexandria, that a very small proportion of the total distance *viâ* Southampton was by land; while the total distance was 3,428 miles, as against 2,429 miles by the shortest possible route across the Alps by the Mont Cenis Railway. What Captain Tyler stated, in the interesting Report which was published last year as a parliamentary paper, with refer-

FOREIGN TRADE of GREAT BRITAIN in 1865, Exclusive of SPECIE.

	Imports.	Exports.	Tonnage of Ships.	
			Inwards.	Outwards.
	£.	£.		
Total	271,131,967	165,862,402	14,317,866	14,576,206
To and from Eastern Seas .	68,117,356	42,897,846	1,492,102	1,869,090

LONDON to ALEXANDRIA.—Distances in English Miles.

VIÂ.		Land.	Water.	Total.	Time.	
					Days.	Hours.
Southampton		75	3,353	3,428	15	0
Marseilles		831	1,701	2,532	8	1
Brindisi	Fast Mails	1,482	977	2,459	6	7
Ditto	Heavy Mails	1,482	977	2,459	7	9

ence to the Brindisi route, was true, viz., that under ordinary circumstances, land conveyance by railway was at least twice as speedy as conveyance for the same distance by water; and on the completion of the Mont Cenis Railway, instead of there being as at present fifteen days between London and Alexandria *viâ* Southampton, or eight days one hour which was the contract time for the mails *viâ* Marseilles, the time of transit would only be six days seven hours, so that the journey would, under an arrangement with the Italian government, be shortened by forty-two hours. This was an important consideration with regard to the postal service. The weight of the outward Eastern mails *viâ* Southampton was in 1865 upwards of 700 tons, and by measurement 1,500 tons; and in 1866 they were still heavier. It was, therefore, of great importance, to England in particular, that a system of railway across the Alps which would give the utmost possible facilities should be carried out. He had no doubt in his own mind, speaking as a person connected with railways for many years, that in the course of the next few months the 1,504 miles, which constituted the distance between London and Brindisi by the Mont Cenis summit line, would be accomplished at an average rate of 20 miles an hour.

ALPINE ROADWAY PASSES.

NAME.	Width.	Summit above Sea Level.	Distance *viâ* London to Brindisi.	Break in Railway.
	Feet.	Feet.	Miles.	Miles.
Semmering	..	2,893	..	..
Brenner	25	4,650	1,769	73
Stelvio	16 to 18	9,272	..	..
Splugen	15 to 18	6,940	..	..
Bernadino	15	7,115	..	..
Lukmanier	..	6,500	..	..
St. Gothard	18	6,808	1,483	146
Simplon	15 to 30	6,636	1,471	114
New Route	..	..	1,435	114
Great St. Bernard	..	8,200	..	..
Little St. Bernard	..	6,780	..	..
Mont Cenis	18 to 30	6,658	1,504	48
Ditto, Tunnel	..	4,118	1,498	42
Mont Genevre	..	5,850	..	..
Col di Tenda	..	5,890	1,833	118
Corniche Road	30	2,200	1,796	127

He had prepared the following table, giving the different carriage roadway passes which now existed across the Alps; and he had included one or two that were not actually carriage roadway passes—as, for instance, the Lukmanier Pass, as, although there was no actual roadway across the mountains by that pass, yet it possessed great facilities for the construction of a railway; and no doubt a line on Mr. Fell's system could be made along that pass. England was more particularly interested in the passes on the western side of the Alps, because it was by means of one or more of them that the nearest communication by railway with Brindisi was to be obtained. It would be seen that, by the Mont Cenis summit line, Brindisi was 1,504 English miles from London, as calculated by Captain Tyler in his Report. When the tunnel, which although it had already been nine years in construction was, on the 15th October last, only half perforated, was completed, a saving of only 6 miles would be obtained at an enormous cost. There was no doubt the cost of the Mont Cenis tunnel would exceed the estimate of Captain Tyler. Sir Cusack Roney believed it would be two-thirds more, and all that cost would be incurred for a saving of 6 miles. In addition to that it would be seen that, though the summit line was at an elevation of 6,658 feet, there was a gain of only 2,500 feet by the tunnel line, in consequence of there being a continuous rising gradient in the tunnel from each entrance to exactly the centre. The nearest route between London and Brindisi would be by the Simplon line; the exact distance by that route being 1,471 miles. With a short line, which would cut off an angle in the railway communication between Paris and Lausanne, there would be a saving of 36 miles, thus eventually reducing the distance between London and Brindisi to 1,435 miles. The only railway now actually existing across the Alps was the Semmering. Its summit was at an elevation of 2,893 feet. An interesting Paper on that railway was presented to the Institution some years ago, and he need not refer to it further.[1] A line of railway through the Brenner pass was in course of construction, and would be opened about August next.

Mr. Conybeare said, as Engineer to several lines traversing the mountain districts of North and South Wales he had had a good deal to do, during the last ten years, with the laying out and working of lines of exceptionally steep gradients. On these railways, the longest continuous length of steep gradient occurred on the Brecon and Merthyr, in descending from its summit level (situated on the mountain range known as the Brecon Beacons) to the valley of the Usk. The descent was 900 feet in 6·62 miles, giving an average gradient of 1 in 38·8, and the curves varied from 25 to 40 chains radius. This portion of the line was laid out in 1856, the plans were deposited in 1858, and the line was opened for traffic on April 19, 1863, but it had been worked over with heavy engines for twelve months previously. The rails weighed 70 lbs. to the yard. The total length of the Brecon and Merthyr at present completed was 89 miles, and a considerable portion of it was on these exceptionally steep gradients. The ascent from Merthyr to the level of Dowlais was 5½ miles at 1 in 49, and in the 20 miles between Dowlais and Brecon there were 6⅝ miles steeper than 1 in 39, and 1 mile 70 chains of 1 in 40, making 8½ miles out of the 20 miles, of 1 in 40 or steeper.

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[1] *Vide* Minutes of Proceedings Inst. C.E., Vol. xv., pp. 349, *et seq.*

As Captain Tyler had observed, the opinion of Engineers regarding the economical limit of steepness of gradients had undergone a great revolution of late years. In closing the last discussion that took place on this subject,[1] Mr. Bidder expressed the opinion that the working expenses alone of a gradient of 1 in 40 with an up-hill load would be from 3*d.* to 4*d.* per ton per mile, and even with a down-hill load, would absorb the whole receipts, leaving nothing whatever for interest on the cost of the rolling stock. This opinion was quoted at public meetings held in 1859, to prove that the Brecon and Merthyr, and the Merthyr and Abergavenny (another line in the district with similar gradients, to which he was the joint Engineer), could not be remuneratively worked. The portion of the line on which the steepest gradients were situated had been used for traffic since the spring of 1862. The particulars of the cost of working, furnished by Mr. Henshaw, the traffic-manager and locomotive superintendent, showed an unusually favourable result, both as regarded adhesion and economy of working. Tank engines were used, having six wheels coupled, of 4 feet 6 inches diameter, with a wheel base of 12 feet; the cylinders were 17 inches in diameter, with a length of stroke of 24 inches. The saddle tank contained 1,100 gallons of water. The weight of the engine in working order was 38 tons. The engines and break vans were furnished with large sand boxes, the supply of which was carefully attended to. The weight of the break van was 8 tons. All the passenger carriages were supplied with Fay's continuous break. The working pressure, 100 lbs. on the square inch, was maintained throughout the ascent. These engines took a regular load of ten loaded wagons and two break vans, weighing altogether 136 tons gross, at the regulated speed of 8 miles an hour, up this gradient of 6 miles 50 chains of 1 in 38.

The engine "Cymbeline" was built by Messrs. Sharp, Stewart, and Co. specially for this line, after a careful consideration of the facts of the case, and of the experience gained in previous working. During the month of December, 1866, this engine ran between Brecon and Dowlais, a distance of 20 miles, out of which there were more than 8½ miles with gradients varying from 1 in 38 to 1 in 40, with the following results:—

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xviii., pp. 69, 70.

Working of the Engine "Cymbeline," December, 1866.

Miles run . .	1,897		
Coal . . .	703 cwt.	41·5 lbs.	per mile.
Oil . . .	45 pints	·023 pt.	,,
Tallow . .	44 lbs.	·023 lb.	,,

The cost per 100 miles, including wages, was 1*l.* 8*s.* $3\frac{1}{2}$*d.*, or about $3\frac{3}{8}$*d.* per train mile, with a net load of 85 tons.

He attributed the high rate of effective adhesion of these engines, and their economy of working, to two causes—first, that they were specially and carefully designed for the particular work they had to do; and, secondly, that there was only one curve sharper than 25 chains radius on the ascent. According to his experience, Captain Tyler adopted too low a figure in taking the effective adhesion that might be relied on in ordinary working at only $\frac{1}{10}$th of the insistent weight. On the other hand, he was altogether incredulous as regarded the alleged instances of a maximum of ·177, or a little over $\frac{1}{6}$th being exceeded, even under the most favourable circumstances. It was well known from the experiments of Morin and Rennie, that the coefficient of friction of wrought iron on wrought iron, where no unguents were interposed, was ·177, or, in other words, that it would require a horizontal force of a little over $\frac{1}{6}$th of the insistent weight to make a mass of wrought iron slide on a plane surface of the same material, and it appeared to be a necessary corollary that, when the resistance of the load exceeded ·177 of the weight on the driving wheels, the latter must slip on the rails. He was aware of the American instances adduced of the alleged utilisation in draught of as much as $\frac{1}{4}$th and even $\frac{2}{5}$ths of the insistent weight; but, on looking carefully into these instances, the data would not be found to warrant such a conclusion. It would be seen that in all the instances, (where really precise data had been given), in which so exceptional a result was claimed, the motor had been a bogie engine; and Mr. Zerah Colburn, in speaking of instances where the effective adhesion was stated to have been as high as $\frac{2}{5}$ths, remarked that this was the nominal weight on the driving-wheel, and that it must be borne in mind "that when an engine was running, especially upon inclines, there were many circumstances to increase the weight on the driving-wheels. The draught through the draw-bar tended to lift the engine from the front wheels, and thus to increase the load on the driving-wheels. In going up an incline, the water rose at the back end of the boiler; the difference of level upon an incline of 1 in 10, being 19 inches in the ordinary length; and the base of the centre of gravity was thrown further back upon the driving-wheels." And that "These circumstances materially affected the running condition of the engine."[1]

The explanation suggested of the apparent paradox of an effective adhesion being obtained, exceeding that due to the coefficient of friction, appeared altogether insufficient. Mr. Colburn said that, "if the engine was making steam freely, and blowing off strongly, the reaction of the escaping steam against the air sensibly increased the weight of the engine." An engine would scarcely be blowing off steam while dragging a load up 1 in 10. The steam escaping from the blast-pipe might indeed have some effect in the direction indicated, but not a very material one. A more usual explanation of the anomaly alleged was, that by the partial sinking of the wheel into the rail (due to the concentrated pressure of the former) at its point of contact, and the elasticity of the material, the wheel obtained a greater grip on the rail than what would be due to friction alone. But this explanation cut both ways; for such partial yielding would necessarily augment the resistance in front, by raising a steeper gradient, so to speak, in front of the wheel. The interposition of sharp sand would, of course, increase the effective adhesion; but as such increment of adhesion could only be obtained at the expense of the structure of the rail, it would not be desirable to rely on the use of sand in ordinary working conditions. He believed that any engine, having all its wheels motors, might in dry weather exert a tractive force of $\frac{1}{6}$th of its weight, and, by the use of sharp silicious sand, might do so in all weathers; but that to obtain a higher effective adhesion than that due to the coefficient of friction was as impossible, as to utilize more than 100 per cent. of the entire theoretic duty of a fall of water.

In laying out a mountain line, on which sharp curves were inevitable, it was always desirable to allow for the increased resistance on such curves, by flattening the gradient where they occurred, and thus to equalize the draught on all portions of the ascent. But unfortunately, in attempting this, Engineers had hitherto worked very much in the dark, owing to the want of authentic experiments on the increment of resistance, due to curves of different radii at varying speeds, with engines and carriages of a given wheel base. It was strange that a series of experiments, admitting of such easy execution as these do, should not yet have been instituted, to determine a matter so important in mountain railway making.

Mr. Latrobe, the Engineer of the Alleghany Mountain Lines described in Mr. Isaac's Paper, published in America many years ago, the following Table of such resistances, stated to have been founded on experiments on a level :—

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xviii. p. 63.

TABLE showing the RESISTANCE DUE to CURVES. By Mr. LATROBE.

Radius in Chains.	Resistance is Equivalent to that of an Ascent of—
40	1 in 1,949
35	1 in 1,747
30	1 in 1,448
25	1 in 1,236
20	1 in 993
15	1 in 734
10	1 in 482
8	1 in 389
5	1 in 248
3	1 in 149

But Mr. Conybeare had found in practice, that the relaxation in gradient prescribed by this table was wholly insufficient to counteract the increase of resistance occasioned by curvature on steep gradients. It appeared, too, that in American practice this table had been found altogether insufficient. In Mr. Isaac's Paper, it was stated that, on the Mountain Top Incline, the gradient was reduced on curves of 300 feet radius, from 1 in 18·87 to 1 in 22·22; and that the relaxation of gradients on such curves (based on Mr. Latrobe's table) was, over the Board Tree Tunnel, 31 feet a mile. This being found insufficient, it was increased on one part of the Mountain Top Incline to 43 feet per mile, and on another portion to 58 feet per mile, and yet even then was found to be insufficient.

"The speed was always diminished on leaving a straight portion of the track, and on entering a curve of minimum radius, although the resistance of gravity, on the latter, was 25½ lbs. per ton less than on the former; a fact evidently proving, that the resistance of the curve must have exceeded 25½ lbs. per ton of engine and train. It is probable, that Mr. Latrobe's experiments give a sufficiently approximate measurement of the increase of friction of the curves, due to carriages of American construction. Assuming, therefore, his allowance of 13 lbs. per ton, as the additional friction of the train, on a curve of 300 feet radius, the additional friction of the engine, due to such a curve, must have exceeded 49 lbs. per ton of its own weight. This friction will of course vary in engines of different construction."[1]

On the American lines the amount of relief, required in the gradient in going round curves, thus appeared to be underrated; but on the Mont Cenis line it evidently had been overrated, as regarded the engine's gross speed on the curves. Perhaps Captain Tyler would state the amount of relief given? It was desirable that a question of such importance as this should be determined by a series of carefully-instituted experiments.

He considered the Cymbeline class as good a type of engine, for working steep gradients with economy, as any in England. It would be an improvement to adopt the American plan, of having four pairs of wheels coupled; and as such engines were used in the United States on curves of much sharper radius than those usual on English lines, there would appear to be no objection to the addition of a fourth pair of wheels on that head. If tank-engines were objected to, the wheels of the tender must be made driving-wheels, either by some system of coupling or by supplemental steam cylinders attached to the tender: the last plan involved a great loss of heat.

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xviii., p. 59.

The sharpest curve he had ever met with was one close to the town of Rhymney, on a mineral line, called the Old Rhymney, which he had converted into a passenger line. This curve he had ascertained, by two measurements, to be 1¾ chain radius. The line was worked by tank-engines, having six wheels coupled, with a wheel base of 11 feet 6 inches, which ground round it at the rate of 3 miles or 4 miles an hour only. The curve by which the Great Northern joined the Metropolitan Railway was 7½ chains radius, on a gradient of 1 in 46; and the North Western had this year deposited plans for an extension to Dowlais, which joined the Brecon and Merthyr at that place, with a curve of 7½ chains on 1 in 40. He thought that at present there was a disposition to underrate the objection to such curves as these.

The results of experiments had proved, that the employment of a single engine, as on the Semmering, gave better results than were obtained by two engines coupled together, as on the Giovi. Mr. Fairlie's plan had all the disadvantages of the Giovi, with the addition of some grave disadvantages peculiar to itself. When tried on the Brecon and Neath line, it had turned out a complete failure, as any expert might have predicted. There was one fire-box to the two engines; thus the escaping products of combustion had two lines of exit to choose between; and it was a fact well known to all who had experience in boiler work, that the products of combustion, on leaving the furnace, took the shortest and hottest path open to them on their way to the chimney; bifurcated flues were, therefore, erroneous in principle. The steam-pipes were always breaking; the driver could not see the state of the fire; and both driver and stoker were without protection from the weather. The great length of the boiler was also objectionable on steep inclines, as it threw too much weight on the trailing-wheels.

In a pamphlet published in 1859, after describing the lines by which the Alleghany mountain had been traversed, he wrote—

> "Why should not similar expedients be adopted at the passes of the Alps, which still continue to occasion so inconvenient a break in the railway communications of Europe? It has often been proposed to traverse three of these passes by railways; one of which has actually been commenced (though grave

doubts are entertained of its being proceeded with), and a large engineering expenditure has been incurred on the plans and surveys of all three projects, each of which would involve at least one tunnel of unprecedented length and difficulty; but these passes are already traversed by roads which do not exceed in inclination the railway zigzags over the Alleghanies; and the alteration of the existing roads at Mont Cenis and the Simplon into railways of this class would be an improvement of European importance, and one that could not fail to prove remunerative to any company or government that would undertake the work."

Mr. C. H. Gregory, V.P., said, Mr. Conybeare had alluded to the variations in the estimates of the amount of retardation arising from curves; but he had omitted to mention one circumstance which would account for the difference, viz., the structure of the engine. With engines having a long wheel base, and rigidly parallel axles, serious retardation was unavoidable on sharp curves; but the use of bogies, radiating axles, and other appliances, had already greatly diminished the resistance arising from curves, and further improvements might be anticipated in the same direction.

He would call attention to some local difficulties which would affect the working of a railway over the Mont Cenis, in regard to which Mr. Brunlees would perhaps give some explanations. During four or five months of the year, the district extending from Lanslebourg over the summit of the pass to Molaret (a village about halfway from the summit to Susa), was covered with deep snow, and the wind frequently formed deep and extensive snow-drifts, varying in extent and position with the force and direction of the wind. During that period the traffic was carried on between the places named in sledges; and those who had travelled by that route would confirm the observations he had made, that, without any long warning, the sledges often came to very deep snow-drifts, through which their progress was slow, and from which, at times, they had to be dug out. Besides the drifts, there were in parts of the distance accumulations of hardened snow, so great that the surface on which the sledges ran was many feet above the surface of the road. The changes in the condition of the snow were very rapid, and a few hours would sometimes suffice to render the pass impracticable for days, and travellers were occasionally stopped halfway on the Pass, and had no means of proceeding or returning. He supposed that few railways were exposed to the effects of snow to so serious an extent, and under such peculiar circumstances. There were railways, both in Europe and in America, subject to heavy falls and long continuance of snow, and then the road was cleared by the use of the snow-plough—often a heavy and tedious operation. He was aware that, to a certain extent on the Mont Cenis line, covered ways were proposed, which he supposed were more particularly intended as a protection from avalanches; but he hoped that Mr. Brunlees would inform the Meeting, what he anticipated would be the effect of heavy snow-drifts, and large accumulations of the snow upon Mont Cenis, in the working of the railway under consideration, how he proposed to meet the difficulties of the case, and what special provisions he proposed to adopt to keep the snow clear from the middle rail, so as to give the necessary adhesion to the horizontal wheels.

Mr. Brunlees wished in the first place to remove any misapprehension that might have arisen. To Mr. Fell, as inventor or perfector of the central rail system, would belong the merit of any success which might be achieved by the Mont Cenis Railway. His own share in the undertaking had been simply to see that the system was properly carried out, and the line efficiently made, and that there was due provision against the accumulation of snow, either by drifting or the falling of avalanches. It was true that there was on certain parts of the route great liability to avalanches; but they had been guarded against as far as possible by the construction of covered ways of masonry. For other portions of the line, comprising the snowy range, where drifts of great depth occurred, timber and iron covered ways were provided; and on the intermediate section, between these covered ways and the avalanche galleries, such observations were now being made as would, it was believed, enable him in the course of next summer to provide screens that would in various places free the line from drifts. From the point where the timber-covered ways commenced on one slope of the mountain, to the termination of the covered ways on the other side, in all about 10 miles, the snow lay for about five months in the year.

With respect to the mode of working the level crossings with the middle rail, the first plan he thought of was that of canting the middle rail over on its side, and bringing it down to the level of the two ordinary rails. To that there were, however, many objections. Mr. Barnes had suggested a method of supporting the central rail at the level crossings on upright bars, hinged at their base and at their connection with the under side of the rail, and which could by a motion similar to that of a parallel ruler, be let down to the same level as the ordinary rails, which were crossed in the usual manner. He believed that this was the best way of dealing with the crossings.

The effect of severe frost was provided against by everything being left free and open so that the ice could not accumulate. The snow-plough he proposed to place on the Mont Cenis Railway was of a form suggested by Mr. Alexander, who had had great experience in dealing with snow in Canada. Various descriptions of ploughs had been adopted by different railway companies, but he believed this would prove the best. The under-part of the plough was fixed, and would clear the snow from the level of the ordinary rails to about the level of the central rail. Then, as it was not allowed to turn the snow on to the turnpike road, which ran parallel to the railway, provision had to be made for shifting the upper part of the plough from the left to the right, or vice versâ, so as always to throw the snow off on to the side of the railway next the precipice. This was effected by means of a rack-movement at the back of the plough, which enabled the machine to be directed to the right or the left as occasion required. He thought that by this or a similar plan any ordinary snow-fall might be got rid of.

The annexed Table (p. 343), which he had prepared, of the available power derived from the various engines hitherto found most effective in working inclines, showed the superiority of the central rail system.

Mr. Pole would offer a few remarks on one or two points that had incidentally been mentioned in the Paper or in the discussion. Captain Tyler's Paper might equally well have been entitled "On the Passage of the Alps by Railways;" and this of itself would form an interesting subject for the Institution. He was surprised to find it so little known in England, that there was already a railway nearly finished across the main chain of the Alps, and, what would probably be thought still more surprising, without any long tunnel or any such very steep gradients as had been referred to in the Paper. This was across the Tyrolese Pass of the Brenner, between Munich and Verona. He had occasion to cross this Pass in 1865, and was amazed to find this railway in progress, of which he had never before heard. Being thus unprepared, he had no means of getting detailed information about it, but he followed its line along the whole length, and would state what he knew. The railway had already been finished up to Innsbruck on the north side, and to Botzen on the south side, the distance between these places in a straight line being about 80 miles by the carriage road, and involving the pass of the Brenner at nearly 5,000 feet above the sea. At Innsbruck the line left the main valley of the Inn, and turned abruptly southward, ascending the small tributary of the Sill, till it arrived at the summit of the Pass, which it crossed alongside the carriage road, without any tunnel through the ridge. From thence it descended the stream of the Eisach, a tributary of the Adige, down to Botzen, where it entered the main valley, and joined the railway already made by Trent to Verona, and communicating there with the railway system of Northern Italy. The rise from Innsbruck to the summit was about 2,850 feet, and the road was 28 miles long; but as the railway made several detours round lateral valleys to gain length, the average gradient would probably be only about 1 in 70 or 1 in 80. The fall from the summit to Botzen was about 3,500 feet, but there was nearly twice the length to do it in. Of course, there were some places where the nature of the ground required steeper gra-

—		Diameter of Cylinder.	Length of Stroke.	Diameter of Driving-Wheel.	Weight in Working Trim.	Weight available for Adhesion.	Adhesive Power taking 450 lbs. per ton (or $\frac{1}{5}$).	Tractive Power at 90 lbs. mean pressure.	Proportion of Weight of Engine to Tractive Power.	Weight on each Driving-Wheel.	Weight which can be taken up 1 in 12, exclusive of Weight of Engine.	
											Adhesion $\frac{1}{10}$.	$\frac{1}{5}$.
		ins.	ft. ins.	ft. ins.	tons.	tons.	lbs.	lbs.	tons.	tons.	tons.	tons.
Mr. Fell's Engine, four wheels coupled.		15	1 4	2 3	17	17	7,650 10,800 18,450	12,000	3·17	4·25	29·6	43·
Mountain Top,	six ,, ,, .	16½	1 8	3 6	24·5	24·5	11,025	11,670	4·70	4·1	3·3	31·5
Oldham Engine,	six ,, ,, .	15	2 0	5 0	29·15	29·15	13,117	8,103	8·06	4·86	3·5	11·
Ditto,	six ,, ,, .	15	2 0	4 0	29·15	29·15	13,117	10,130	6·44	4·86	3·5	21·5
Fairlie's ditto,	eight ,, four cylinders	15	1 10	4 6	42	42	18,900	16,507	5·70	5·25	5·7	41·
North London,	four wheels coupled .	17	2 0	5 9	42	30	18,500	9,050	10·39	7·5	0·0	3·2
Oldham Goods,	six ,, ,, .	17	2 0	5 0	49	33·65	15,142	10,410	10·54	5·6	0·0	3
Semmering,	eight ,, ,, .	18·7	2 1	3 7¾	55·25	55·25	24,862	18,000	6·87	6·9	7·4	35·
Giovi,	eight ,, four cylinders	14	1 10	3 6	55·25	55·25	24,862	18,500	6·69	6·9	7·4	35·

dients; but he was informed the steepest did not exceed about 1 in 40, which was quite practicable for locomotives of ordinary construction. There were several short tunnels through spurs, and a good deal of heavy rock work, but he did not observe any extraordinary difficulty in the line. It was far advanced when he passed it; and he was informed the works had been vigorously continued since, and that the line was to be opened towards the end of the present summer, when there would be direct railway communication between cis-alpine and trans-alpine Europe.

The Brenner Railway was of high importance, both in a commercial and a political point of view. It had no doubt been encouraged by the Austrians, as it would have been of immense value to them when they possessed Venetian Lombardy; and even now, by its passing through the heart of the Tyrol, it was a useful line to them. But it was also looked forward to with interest by the Italians, as being likely to facilitate their commercial relations with Germany and Northern Europe generally. To Great Britain this line was of much consequence, as it opened up a railway communication (*viâ* Ostend and Brindisi) with the Mediterranean and the East, independent of France. The Mont Cenis Line, when finished, would be rather the shorter; but if at any future time political complications unhappily closed that line to English traffic, the Brenner route would be of vital importance; and even in time of peace, it would be a question whether its little extra length might not be more than compensated for by its better gradients and the absence of the long tunnel. If any members of the Institution connected with Italian railways could obtain detailed particulars of this, the first, and probably the best railway across the Alps, he was sure they would be very acceptable.

The atmospheric system of propulsion had been alluded to as a means of crossing Alpine Passes. Mr. Pole had had occasion lately to look into the history of this system,[1] and he would offer a few observations on this application of it. The system had been tried somewhat extensively some years ago, and had failed to establish itself; but there was a good deal of misapprehension as to the causes of this failure. Many people supposed it was from mechanical defects, and that the whole thing was a delusion. But the facts, as far as he could ascertain them, did not bear out this view. Mr. Robert Stephenson, who was one of the most determined opponents of this system, never called its mechanical efficiency in question; and Mr. Bidder had declared that he considered the mechanical problem as effectually solved. In some of the applications in this country, the system worked the traffic regularly for a considerable time; but the most extensive trial was on the St. Germain Railway, near Paris, where it worked successfully from 1847 to 1860.[1] The chief reason of the non-success of the atmospheric system where it had been tried, was that it was not suitable, as a system, for general railway traffic. Mr. Stephenson predicted this, but he always admitted that there were exceptional cases where it might be applied with advantage. Such cases appeared to be occurring in the present day; one might probably be for underground lines, where the use of the ordinary locomotive was objectionable; and another was on exceptionally steep gradients, such as those necessary in carrying railways over high abrupt mountain passes. Under these latter circumstances, the atmospheric system appeared to offer several most important advantages; as for example,

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[1] *Vide* "Life of Robert Stephenson," by Jeafferson and Pole, vol. i., chap. xiv. London, 1864.

1. It would allow of the convenient application of any amount of power that might be necessary, and therefore would admit the use of any gradient desired.
2. The great power necessary to drag the locomotive and its supplies of water and fuel up the incline would be saved.
3. The use of a locomotive in such a situation would considerably enhance the dangers otherwise due to the steep inclines and sharp curves; but the atmospheric system would give absolute safety, both in ascending and descending.
4. The atmospheric system would also offer a feasible means of making use of the large water power generally found in such localities.

It might be objected that in such inclement situations as the Alpine Passes, the valve of the atmospheric tube would be liable to derangement; and this objection was doubtless entitled to grave consideration, but he conceived it was not insuperable. Many competent judges were of opinion that in any case a railway at such altitudes must be covered in, if it was to work during the winter season, and if so, this would at once make atmospheric propulsion practicable, either on Vallance's plan, or on that heretofore used.[2]

Something had been said as to the use of steel in locomotives. Mr. Pole was inclined to fear that this metal had lately been employed for railway purposes somewhat too hastily and inconsiderately, and he was inclined to hope, from the remarks of Mr. Alexander, that a wholesome re-action was taking place. Mr. Pole had had occasion to watch carefully great numbers of attempts to substitute steel for iron in armour plates and guns, but these trials had only shown how unfit the metal (at least in its present state of manufacture) was for purposes where it was exposed to sudden shocks and concussions. It was frequently supposed that because steel had greater tenacity than iron, it was therefore proportionately stronger; but this did not necessarily follow, except for strains perfectly quiescent and purely statical. When motion was introduced, and when shocks and concussions took place, simple tenacity was by no means the measure of strength; for a metal of high tenacity might be very brittle and unyielding, and therefore much less fit to withstand such concussions than a less tenacious material that was softer and more ductile. This seemed to be the case in a large degree with steel, as compared with good iron. In cases where hardness and durability under wear were the principal desiderata, as in tires, no doubt steel was very suitable; but even in these cases much caution was necessary. Instances had occurred, within Mr. Pole's own knowledge, where steel tires had been broken by the wheels being rather roughly handled in carriage. This was not caused by any fault of the tires (which were made by one of the best houses), but was due to their being shrunk on too tight, which showed, however, how much greater caution was necessary with this material; and that, without such caution, steel might prove worse than good iron. To guard against this, some of the steel-makers were now producing so soft a quality that it could hardly be distinguished from iron, and in this state its advantages over the more simple and well-known material became problematical. He could not further yet satisfy himself that the manufacture of steel, under the large production and cheap price at present aimed at, had attained such certainty as to render it trustworthy, in cases where its failure would involve serious consequences. All these matters were well worthy of investigation, and he hoped they would have the careful attention of such members of the Institution as were in a position to obtain information upon them.

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[1] *Vide* Perdonnet, "Chemins de Fer," vol. ii., chap. xi., page 348, 2nd edition, where M. Flachat, the engineer of the line, recommends the use of the atmospheric pressure for inclined planes. He says, "Le chemin de fer atmosphérique de Saint Germain n'a jamais failli. . . . Jamais un accident ne s'est produit; la sécurité du service y est absolue; sa félicité est telle, qu'il me semble mériter à ce titre l'attention la plus sérieuse des ingénieurs." This was in 1860, after nearly fourteen years' trial.

[2] Since making these remarks, Mr. Pole has had an opportunity of seeing a pamphlet, by Mr. George Edwards (Systême de Chemin de fer Hydro-pneumatique pour le passage des Hautes Montagnes, Turin, Octobre, 1865), in which the practicability of working Alpine Passes by atmospheric pressure is fully discussed and warmly advocated. It is not published; but will be found in the library of the Institution.

Mr. G. W. Hemans, in reference to what had fallen from Mr. Pole with regard to the atmospheric system, stated he had been engaged in considering the mode of traction to be adopted on an Alpine railway, part of which had been for some years completed and in operation. Amongst other modes he had considered the atmospheric system, but he found that frost and snow would prove fatal obstacles to it. The main principle of that system was a tube, of the same length as the railway, which had a continuous valve along the whole of its length. That valve was hinged upon leather, and the adhesion of the flap of the valve to the other side of the opening was effected by grease or a glutinous substance; but frost and snow destroyed the adhesion between the valve and its seat, and the whole, under such circumstances, became useless, as the vacuum could not be maintained in front of the moving piston. The atmospheric system was tried on the Dalkey branch of the Dublin and Kingstown Railway, which had succeeded at first, but the effects of frost, in conjunction with the heavy cost of that system of traction, had led to its discontinuance.

Mr. Wm. Naylor thought it quite possible that on the Mont Cenis line, the plan suggested for dealing with the central rail at level crossings might be simplified, and that the middle rail might be lowered down to the level of the ordinary rails, so as to allow vehicles to pass over it, gates being provided on each side to prevent anything crossing when a train was due. He would remark that he did not see any provision for shunting. He apprehended it was necessary that there should be some means of getting one train past another; and this he thought could be accomplished by making a siding on the steep part of the incline, when the horizontal wheels could be lifted above the rail, and a portion of the middle rail be made into a switch to clear the way for the wheels to get from one rail to another. In ascending the incline, there must be an expenditure of power equal to taking the train up a certain distance, and raising it from one level to another, no matter what the motive power was. It was also necessary to look carefully to the means by which the train could safely descend the incline. If the rope system or the atmospheric system were adopted, and a snow-drift occurred with the train going down the incline, it would not require much to compress the snow so as to lift the flanges of the wheels above the line and allow the train to make a track for itself: and that might lead to serious consequences where there was a deep valley for the train to fall into. The middle rail, however, was a guard against such a casualty. It was well known what difficulty there was in driving a train through deep snow in this country; and he felt, on that account, it would be impossible to get a train up these inclines without more adhesion than was afforded by the bearing-wheels upon the ordinary rails; and that was in a great measure obtained by the extra adhesion of the middle rail. At the same time it was important to see what could be done with inclines in climates where snow did not exist. Captain Tyler had stated, from information he had received from the manager of a railway in Wales, that an incline of 1 in 17 was worked by an engine of 36 tons weight, in working order, with wheels 4 feet in diameter, cylinders 16 inches in diameter, and a length of stroke of 24 inches, and with a pressure on the boiler of 130 lbs., the regulated load being 25 tons. That seemed to him to be a light load; and from inquiries he had made about the same incline, he had ascertained that the engine weighed in working trim 36 tons, and that the fixed load was 40 tons, but on one occasion the locomotive superintendent had himself taken up 45 tons. The Lickey Incline, of 1 in 37, had been worked for a number of years with engines weighing 35 tons, which took up loads of 140 tons, or nearly four times the weight of the engine; the gravity on that gradient was $60\frac{1}{2}$ lbs. to the ton, and taking $9\frac{1}{2}$ lbs. or 10 lbs. for friction, to meet the force of a strong wind, or other sources of resistance, then the total resistances would amount to 70 lbs. per ton. That required a tractive force of 12,250 lbs. at the periphery of the wheels, which were 4 feet in diameter, the cylinders being $16\frac{1}{2}$ inches in diameter, and having a length of stroke of 24 inches. It would be found, therefore, that it would require 90 lbs. pressure of steam throughout the entire stroke to give that amount of traction, and that the engine at that pressure would take 140 tons up 1 in 37; 50 tons up 1 in 17; and $27\frac{1}{2}$ tons up 1 in 12. If this were correct the same engine with 112 lbs. pressure of steam would take 183 tons up 1 in 37 (exclusive of the engine); 73 tons up 1 in 17; and 42 tons up 1 in 12. It might be said that 112 lbs. was rather a high pressure; but on the Vale of Neath Railway there were two tank engines, with cylinders $18\frac{1}{2}$ inches in diameter, and having a length of stroke of 24 inches, in which a pressure of 112 lbs. to the square inch was found to be necessary, in order to take a gross load (engine included) of 300 tons up 1 in 47, the wheels being 4 feet 6 inches in diameter. If that could be done in this case he saw no reason why the same thing should not be done on the Lickey Incline. The coefficient of adhesion was taken at one-fifth. With an eight-wheeled engine, the cylinders being 18 inches in diameter, with a length of stroke of 24 inches, the wheels being 4 feet in diameter, and the pressure of the steam 110 lbs. to the square inch, a load of $83\frac{1}{2}$ tons (excluding the weight of the engine, 40·4 tons) could be taken up 1 in 17, and $46\frac{8}{10}$ tons up 1 in 12.

As to the relaxation of the gradients on curves and the amount of resistance due to curves; assuming the chord of a curve as 15 feet, and the versed sine as $\frac{1}{16}$th of a foot, or $\frac{3}{4}$ of an inch, giving 450 feet radius, with a six-wheeled engine with the wheels 7 feet 6 inches apart, and $\frac{3}{4}$ of an inch play on the line, which was usually allowed, the fore and aft wheels would be close to the outer rail, and the flange of the middle wheel to the inner rail. With an eight-wheeled engine, having the same aggregate wheel base, and wheels 4 feet in diameter, the two end pairs being 4 feet 3 inches apart, and the middle pairs 6 feet 6 inches, giving a total wheel base of

15 feet, then with $\frac{1}{2}$ an inch of lateral play in the axle-boxes each way, in the leading and trailing axles, as on the Vale of Neath Railway in the case of these heavy engines, and as adopted by Mr. George Berkley for the Great Indian Peninsula Railway, such an engine would travel round a curve of 225 feet radius (or half the other) without the wheels jamming. In fact he might state, that one engine made for the Great Indian Peninsula Railway went freely round a curve of 200 feet radius; but that was merely in the workshop. On the question of what was the amount of friction on curves: the inner rail was 4 inches shorter than the outer rail, on a wheel base of 15 feet—the gauge being 4 feet $8\frac{1}{2}$ inches, and the radius of curvature 225 feet—and assuming the weight for adhesion to be 1 ton on each of the four wheels which rested on the inner rail, 4 tons would have to slide along 4 inches of rail in traversing this length (15 feet) of a curve of 225 feet radius. To find the increase in the resistance per ton of load to the tractive force due to such sliding motion, the 4 tons should be multiplied by 4, and be divided by 180 (the number of inches in 15 feet), when the result would be found to be 5 lbs. This amount of increased resistance due to the curvature would be important on a level, as it represented about 60 per cent. of the total force required to propel 1 ton on a straight line on a fair day. But on a steep incline of say 1 in 12, on which the gravity of 1 ton was equal to $186\frac{1}{2}$ lbs., or, with the addition of friction, the total resistance was equal to about 196 lbs., it would be comparatively unimportant, as it would amount to only $2\frac{1}{2}$ per cent. of the total force.

Mr. W. Lloyd had been engaged during the last twelve years in the construction of some of the principal steep inclines in the world. Most of the illustrations and ideas connected with this subject had been derived from Europe alone. His own experience was that many things which were good in Europe frequently caused a great deal of difficulty in foreign countries; and there was one thing in Mr. Fell's engine, admirable as it was, which struck him would cause difficulty in practical operation on a railway abroad—that was the extreme intricacy of the machinery. Simplicity was an essential element in all things connected with a railway in countries distant from the great centres of industry. He had never doubted that the Alps might be crossed by a railway. The notion was also entertained of going over the Andes, and if that were done the altitude achieved by Mr. Fell and Mr. Brunlees would be greatly surpassed. There would not be the same difficulty with regard to the line itself, but the elevation to be reached was nearly double. It was found in the investigations of the Andes, that there would be no necessity to approach an inclination anything like that of 1 in 12. In the majority of cases a gradient of 1 in 20 or 1 in 25 was the utmost that would be required. The line with which he was principally connected in Chili was the Valparaiso and the Santiago Railway; and he alluded to it more especially because he thought practical results could not be too often referred to. The theoretical features of a scheme might be discussed; but those who were connected, as he had been, with the practical working of railways, as well as with their construction, knew that the results of actual experience afforded the best guide. He had stated in a Paper read before the Institution,[1] that in his opinion it was most essential that engines of the ordinary class and all of the same class should be used on distant railways abroad. Directly two classes of engines on such railways were introduced, the difficulties in the workshops were quadrupled. He knew instances in which an engine of 40 tons had carried a load of 163 tons up 1 in 50 at 10 miles an hour, or four times its own weight on the driving-wheels. The maximum gradient on the Valparaiso line was 1 in 44, and an engine of 37 tons carried 74 tons load up that at 10 miles an hour, or twice its own weight on the wheels. These figures were easily remembered, and he had no doubt the effective duty of the locomotive would be increased by improvements in the machinery. He was delighted to hear that a load of 40 tons could be taken up an incline of 1 in 12, because many expedients had been tried to get over the difficulty of so steep an ascent.

He would now advert to the Copiapo Extension Railway, from Pabellon to Chanarcillo, for which Mr. E. Woods, M. Inst. C.E., was the consulting Engineer in this country. The summit at Molle was upwards of 4,450 feet above the sea, and Pabellon, where the line commences, was about 2,200 feet above the same level. It had been in operation since February, 1861, and was worked by Messrs. Hawthorn's engines. It was 26 miles in length, and the cost, including equipment, was £6,500 per mile. It consisted of three inclined planes, one rising 2,276 feet in $14\frac{1}{2}$ miles, with an average gradient of 1 in 33, but for short distances of 1 in 26, and 1 in 28 over a portion of the incline. The second was a descending gradient from Molle to Pajonales, 1,990 feet in $9\frac{1}{6}$ miles, with an average gradient throughout of 1 in 24. On one portion, however, there was a gradient of 1 in 20, with curves of 720 feet radius, and on other gradients of 1 in 21 and 1 in 24 there were curves of 490 feet radius, in some cases reversed two or three times, and without any intermediate straight line. The last-mentioned inclined plane was an ascending one, with gradients of 1 in 34 and 1 in 25, in the direction that the chief loads were carried, the principal traffic being from the silver mines to the coast. He was sent by the government of Chili to determine the kind of engine that was best adapted for working such lines. The train he took up weighed 57 tons; it went over the gradient of 1 in 20, with curves of 720 feet radius, without difficulty. The retardation by the curves was not more serious than could be overcome by moderating the weight of the train; and there was never any occasion for the weight to exceed 70 tons or 80 tons, as the traffic did not require heavier trains than that. The cost had generally to be regarded in working steep inclines; but in a country like that of which he was speaking it did not enter into the calculation, inasmuch as the price paid for the transmission of goods was so high, that almost any cost would be more than remunerated by the rate of prices charged. The engines had six coupled wheels, 4 feet in diameter; the cylinders were 16 inches in diameter, with a length of stroke of 24 inches; and the fuel burnt was, at the time he spoke of, about an equal quantity of coal and coke. The paying load was two-thirds of the weight of the train. The consumption of fuel was 75½ lbs., and of water 66½ gallons, per mile. He considered one-fourth of the weight on the driving-wheels was fairly taken up by the engine.

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxiii., p. 376.

Subsequently a trial was made on another incline in Chili of 1 in 13, over a tunnel under construction, with an engine of the ordinary character of those which were being used in the country at the time; but it was found that the ordinary class of engine could not be utilized for such a gradient as 1 in 12 or 1 in 20. He was, however, reluctant to make special arrangements for a locomotive to work that small portion of the road temporarily; he therefore took a four-wheeled engine off its wheels, placed it as a stationary engine on the top of the tunnel, and worked the incline by a rope. By that means four heavy trains were worked daily; and that during four years, with scarcely any difficulty or accident occurring. Such an instance he thought would afford a useful precedent to Engineers under similar circumstances in foreign countries.

He would now briefly allude to what was proposed on the Mexican Railway, because he had heard some doubts expressed as to what an engine would be likely to do on that line. The proposition was this: The altitude to be reached was 8,400 feet above the sea; and for a distance of 23 miles the average gradient was nearly 1 in 25, with curves of 350 feet radius, and there was one length of 15 miles almost continuously of this gradient. The engine proposed was a double-tank engine, with tender and auxiliary power between. The object of the tender was to carry the necessary supply of water, none being obtainable at the top of the incline. The water from the tender was to be first exhausted, leaving that in the tanks to assist adhesion in the ascent. He calculated that two engines with 21 tons on each pair of driving-wheels, or 42 tons together, ought to take up a train of 100 tons, independently of the weight of the engines; and that would be the maximum load required, inasmuch as the present traffic amounted to only about 80 tons per day. As the rate charged was a shilling per ton per mile, the money value of a train of 100 tons for 15 miles would be £75, so there was little need to take into consideration the cost of working such inclines under similar circumstances.

Mr. Beyer said, with regard to friction, he was satisfied the adhesion was about the same under all circumstances, if the rails were in good condition. He had taken some trouble to collect what might be considered reliable information on this subject, and had obtained the results of many experiments. He was convinced in his own mind that, under a good condition of the rails, the adhesion of the wheels was at least one-fifth, and probably as much as one-fourth. When the rails were in bad condition, whatever the weight of the engine, the wheels would slip. With regard to inclined planes, he did not know that it made any difference to the locomotive whether it had to do its work on a level or up an incline; it was simply a matter of how much cylinder power was necessary to pull the load.

Mr. E. Woods could corroborate what had been said with regard to adhesion under favourable circumstances; and it was confirmed by the results of Mr. Lloyd's practice in Chili. Table No. 1, page 353, showed the actual work done on the railway of which Mr. Lloyd had given a description, and which had now been worked by locomotive power for six years. He had stated on a former occasion,[1] that this line was not originally designed for locomotives, but was intended to be worked by animal power. It was so worked for two or three years, but the expense caused it to be abandoned, and locomotives were substituted. The lightness of the rails (42 lbs. to the yard) compelled him to design engines in which the weight on the driving-wheels should not be greater than those rails would stand, and the engine he had designed (Table No. 2, p. 353) had outside cylinders, six coupled wheels, 4 feet in diameter, with a four-wheeled bogie in front, to pass round curves of 500 feet radius. The weight of the engine, in working order, was 32 tons, and that of the tender 25 tons: on the driving-wheels it was 24 tons. A comparison of that with the work of the engine given in Table No. 3, p. 354, showed that the coefficient of adhesion was as nearly as possible one-fourth of the driving weight. The climate of Chili was, however, peculiarly favourable for working steep gradients. There was very little rain at any time, and the rails were almost always in good order. The line had been worked four or five years entirely without accident, until about half a year ago, when one occurred owing to the breaks not acting. Mr. Lloyd had correctly stated that the chief loads were from the mines to the coast. They were carried on a gradient averaging 1 in 23, or 1 in 24, up to Molle, before they could be sent on to Pabellon; thence they were carried along the Copiapo Railway (extending for a distance of 60 miles) to the coast. The load had on one occasion been 77 tons, exclusive of the engine and tender, making a total of 134 tons. Under ordinary circumstances, the weight of the load was not more than 50 tons, the total being 107 tons. The Tables which he had prepared, independently of Mr. Lloyd's observations, and without communication with him, were confirmed by that gentleman's personal experiments on the line. The speed was necessarily limited in descending: the rule was that the trains should not descend at a greater speed than 12 miles an hour; but that was sometimes a little exceeded. The average speed up and down was about 13 miles an hour. A question had been asked as to the relative amount of coke and coal used for fuel in working these trains. The proportion of coal was greatest. In the first instance coke alone was burnt; then coke and coal mixed together; and now coal was almost exclusively used. He thought a well-proportioned 'all-coupled' engine could take itself up an incline of 1 in 4 with its own wheels, if the rails were clean and in good order.

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxiv., p. 379.

COPIAPO EXTENSION RAILWAY, 1860.

Table I.

Particulars of Engines.

Outside cylinders.	Six coupled wheels, 4 feet diameter.
Four-wheeled bogie.	Cylinders 16 ins. diam., and 24 ins. stroke.

Weight of engine in working order . .	32 tons.
Ditto tender " " . . .	25 "
Total . . .	57 "

Table 2.

Loads taken over the line, gradients averaging 1 in 23 and 1 in 30.

Heaviest train ever taken over the line:—

Engine . .	32 tons.	
Tender . .	25 "	
		57 tons.
Gross load of wagons and carriages .		77 "
Total weight . . .		134 tons.

Ordinary trains :—

Engine . .		32 tons.	
Tender . .		25 „	
			57 tons.
Mineral trucks .	Tare	11 tons.	
Minerals, &c. .	Nett	26 „	
		37 „	
Passenger carriages, with passengers		13 „	
			50 „
Total			107 tons.

Mean speed of train, $13\frac{1}{3}$ miles per hour.

Heating surface :—

Fire-box	75 square feet.
Tubes	926 „
Total . . .	1001 „

Area of grate $14\frac{1}{4}$ square feet, or $\frac{1}{70}$th of heating surface.

Table 3.

Greatest load taken up gradient of 1 in 23 = 134 tons.

Gravity . . .	97 lbs. per ton.
+ friction . .	12 „ „
Total . .	109 „ „

Traction = 134 tons × 109 lbs. = 14,606 lbs.
24 tons (= 53,760 lbs.) on driving-wheels.

$$\text{Coefficient of adhesion} = \frac{14606}{53760} = \frac{1}{3\cdot 7} = 0\cdot 27.$$

Ordinary load taken up, 1 in 23 = 107 tons.
Traction = 107 tons × 109 lbs. = 11,663 lbs.

$$\text{Coefficient of adhesion} = \frac{11663}{53760} = \frac{1}{4\cdot 6} = 0\cdot 22.$$

In the 1st case,
14,606 lbs. traction corresponded to a mean pressure in the cylinder of 114 lbs. per square inch.

In the 2nd case,
11,663 lbs. traction corresponded to a mean pressure of 91 lbs. per square inch.

Mr. Vignoles remarked that extensive experiments, as to the friction of iron upon iron, showed that the utmost amount of adhesion that could be got, under the most favourable circumstances, was one-fifth. He appealed to mathematicians whether he was not correct in stating, that the amount of adhesion, on railways deviating from the horizontal, depended also on the sine of the angle of the inclination of the plane.

Mr. Callcott Reilly said, in reply to an observation made by Mr. Vignoles that, upon a gradient, the pressure of the wheels of a locomotive upon the rails would be less than the weight of the engine.

The normal component of the weight of the engine would be the product of the weight into the cosine of the inclination of the gradient, and therefore the adhesion on the gradient would be diminished in that proportion. It might be worth while to see what would be the amount of that diminution in a practical case. Suppose an engine, weighing 24 tons, to stand upon a gradient of 1 in 20, all the wheels being coupled. Then the diminution of the pressure normal to the rail, owing to those circumstances, would, upon that gradient, be only 70 lbs.; and upon a gradient of 1 in 12, the diminution would be only 201 lbs.—of course a most insignificant trifle. He had supposed the case of an engine having all the wheels coupled; but in the very common case of an engine having its leading wheels uncoupled, or with its front end supported by a bogie, the diminution claimed by Mr. Vignoles would be much more than counteracted, by the shifting backward of the centre of gravity of the water in the boiler, thereby transferring a part of the weight from the uncoupled leading wheels, on to the coupled drivers; so that the adhesion of such an engine would be greater upon any workable gradient than upon a level. Mr. Conybeare had called attention to the coefficients of friction, and gave 0·177 as the coefficient of friction of wrought-iron surfaces, from which he inferred that the adhesion of an engine could not possibly exceed about one-sixth the weight of the engine. He supposed Mr. Conbyeare must have quoted that coefficient from the Tables of General Morin. But it was well known that Morin's results were obtained with pressures of very light intensity, in no case exceeding 30 lbs. per square inch. The actual figures deduced from Morin's Table No. 68 as the average were about 0·138, as the coefficient of friction of motion, with dry

TABLE 4.

"FRICTION OF REST" OF WROUGHT IRON UPON WROUGHT IRON, from Mr. Rennie's Experiments, Phil. Trans., 1829: showing how the coefficients of friction increase with the increase of pressure. Surfaces dry.

Pressure per Square Inch.	Coefficients of Friction.
lbs.	
32·5	0·140
186·0	·250
224·0	·271
261·0	·285
298·0	·297
336·0	·312
373·0	·350
410·0	·376
448·0	·376
485·0	·395
522·0	·403
560·0	·409

surfaces; and 18 lbs. per square inch as the intensity of the pressure.[1] Of course there could be no analogy between those results and the friction resulting from the pressure of a locomotive upon the rails, the intensity of which would probably be 1,000 times as great as in Morin's experiments. It was well known that the late Mr. George Rennie made a series of experiments,[2] with much greater pressures than those of General Morin. These experiments, Table No. 4 (page 355), showed that when the intensity of the pressure was 32·5 lbs. per square inch, the coefficient was 0·140; but when the pressure was increased to 560 lbs. per square inch, the coefficient was 0·409; the coefficients increasing steadily with the pressures, although not in direct proportion. Referring to the best performance of Mr. Woods' engine, it would be seen that its coefficient of friction was 0·27, which was much less than Mr. Rennie's higher coefficients.

Mr. Phipps observed, with regard to passing over mountainous regions by means of railways, that it was of course always to be expected that numerous steep inclined planes and sharp curves would have to be introduced, and hence arose the necessity of dispensing as much as possible with all unnecessary weight in the engine and train. Every one, therefore, whose attention was turned to this subject naturally looked at first with favour upon systems, such as haulage by ropes, the atmospheric railway, &c., which accomplished the necessary tractive power without the necessity of dragging up a heavy locomotive along with the train. This was the purely theoretic view of the matter; but, in the practice of mechanical engineering on a large scale, every-day experience showed, that some compromise must be made between abstract theory and general convenience—just as in the designing of ships, no one single property could be attained in its highest degree of excellence. In the same manner the locomotive engine often came to be used in situations where it was certainly not economical, motives of convenience in its use often outweighing the deficiency in economy. If, for instance, upon some mountain line of railway, several tolerably level portions of the line alternated with steep gradients, the delay and inconvenience of resorting to rope traction at each of those changes would be very great. The atmospheric system had also, by pretty general consent, been admitted to be unfit for regions in which the changes of temperature were so great. He thought that the system of Mr. Fell was a good compromise, between a heavy engine capable of dragging itself and the train up the ascent by the adhesion due to gravity alone, and the other methods before referred to, where the retarding effect of the weight of the propelling power was altogether dispensed with. As an instance of the sacrifice of purely economic views sometimes found convenient to be dispensed with, he would refer to the case of the Metropolitan Railway, where engines weighing about 45 tons were used to draw trains of a total weight, including the engine, of about 90 tons. Hence the net power to draw the useful part of the train was doubled; but this was not all, because, in consequence of the numerous stoppages upon the above line, in order to obtain a comparatively moderate average speed, it became necessary to get up the speed from a state of rest in a very brief time after stopping at each station. Supposing the requisite ordinary tractive power of such engines on a level to be ½ a ton, to get up a speed of 10 miles an hour from rest, if acquired in the length of 100 yards, would require an additional tractive power of about 1⅛ ton, making altogether 1⅝ ton of tractive power, where the useful load of 45 tons would only have required about ¼ of a ton; and yet, up to the present time, no one had been able to propose a satisfactory substitute for the locomotive engine in the above situation.

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[1] *Vide* Nouvelles experiences sur le frottement, 1834, p. 68.

[2] *Vide* Phil. Trans., 1829, p. 159.

He would now pass to a part of the subject of the resistances to railway trains, which had engaged attention during this discussion, namely, the resistance due to curves. There appeared to be a desire to draw a comparison between the resistance on inclined planes and that due to curves, with the object of enabling those engaged in laying out such lines to render the resistance of the trains as nearly uniform as possible. He was afraid that any attempt to calculate the resistance due to curves on purely theoretical considerations would prove a failure. He had attempted to do so on the principle of taking the slip of the wheels on the exterior diameter of the curve (those on the interior diameter being supposed to travel with the velocity of the train) to be equal to the distance travelled by the train in any short space of time, taken into the fraction expressed by the gauge of the line for a numerator and the radius of the curve as a denominator. Then ½ the weight of the train into the fraction for adhesion, into the above distance, would represent the power consumed on the curve. But the result was so exceedingly small as to admit of scarcely any comparison with the effects of inclination. For instance, in the case of a gross load of 33 tons, on a gradient of 1 in 12, on a curve of 3 chains radius, taking the adhesion at $\frac{1}{6}$th of the weight, the result would be:—

$$\frac{\overset{\text{Tons.}}{33} \times \frac{1}{6} \times \frac{1}{55}}{2} = \overset{\text{Ton.}}{0 \cdot 05}.$$

The gravitation down the plane would be—

$$\frac{33}{12} = \overset{\text{Tons.}}{2 \cdot 75}.$$

He was thus satisfied that there were causes for resistance on curves far more influential than the above. This subject had been examined in a Paper by Mr. Isaac,[1] in which it was stated that the resistance to traction due to a curve of 400 feet radius was supposed to be double the resistance on a straight line on a level.

Mr. Bramwell said he rose with some diffidence, after the remarks of Mr. Phipps, for he was about to address a few words to the meeting on the subject of the increased resistance on curves; but as that gentleman had said he could not arrive at a satisfactory solution of the cause of the increase, it might be presumption in him to attempt to do so; he would, however, venture a few observations on the subject. An opinion had been expressed, that the principal cause of the resistance on curves was the different distances travelled by the inner and outer wheels; and it was shown that in a distance of 15 feet, the difference, with a particular curve, was 4 inches. It was then alleged that, by taking the friction of iron upon iron, and ascertaining from this the resistance due to the weight of the engine, the power required to make a slip of 4 inches, while the engine travelled this 15 feet, could be ascertained. But Mr. Bramwell thought there was some error in this method of viewing the matter; because if, for example, it were assumed that the outer wheels were going at the right speed due to the distance travelled, and the inner wheels made all the slip, it was not right to multiply that slip by the whole weight of the engine, because half was on the wheels that were going the right pace. If, on the other hand, the slip were multiplied by the whole weight of the engine, it would have to be assumed, either that the centre of the engine was making half of the total slip, or else, if the centre of the engine were supposed to be going at the true mean pace, then the wheels would each make one-half the total slip, the outer wheels going too slowly, and the inner wheels too quickly. Another error in the calculation was, that no account had been taken of the correction obtained by the cone of the wheels, which, if there were a little play, to a certain extent reduced the slip; but no allowance had been made for that. It was evident, however, that the effect of the cone varied with the diameter of the wheel; for, if the wheel were of small diameter, and the angle of the cone the same as in a wheel of large diameter, the amount of ease given by the cone would be less in a large wheel than in a small one; for the cone gave only a constant difference, whatever might be the circumference, and the proportion of this constant to a large circumference was, of course, less than to a small circumference.

But, irrespective of these considerations, which tended to show that the resistance on a curve was less than it was thus made to be, and which therefore increased the difficulties which had been indicated, he thought it could be shown that there was another cause of resistance, the effect of which would be very considerable, and which was irrespective of the difference between the lengths of the inner and outer rails on a curve.

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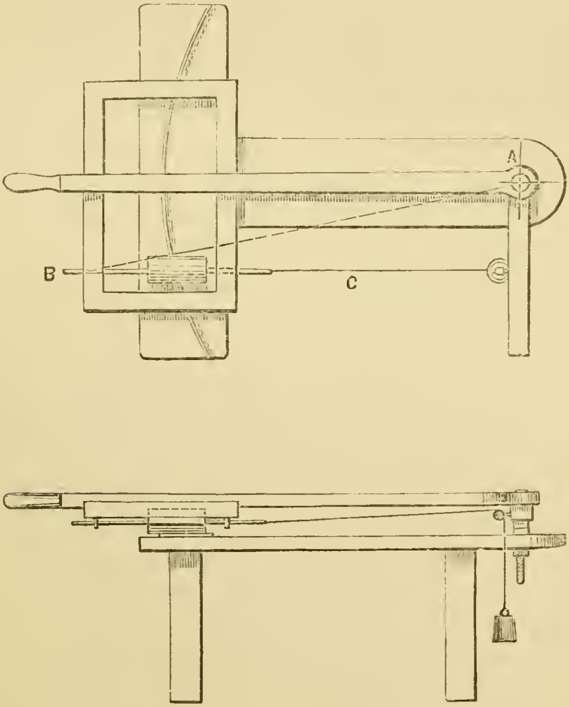
[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xviii., p. 59.

He had represented in a model only a single rail struck to a sharp curve; and he had connected the framework representing the engine or carriage to a radius arm centred at the point from which the curve was struck, and, for convenience, he had taken a roller of some length in the direction of the axle to represent the wheel. Now, if the axle were made radiating from the centre of the curve, as at A B, Fig. 1, the carriage would progress with no more resistance than it would have in a straight path. If, however, as was the case in an ordinary engine, the axle were not radiating, as at B C, Fig. 1, but merely parallel to a radius (the radial arm in the model), and therefore pointed to one side or the other of the centre of the curve, then it followed that the tendency of a wheel on such an axle would be to progress in a direction at right angles to the axle, and therefore it would tend to run out of the curve, and, if left at liberty while the carriage was compelled to follow the curve, the wheel would move endways in the carriage in the direction of the axle, and the successive points of contact of the wheel

Fig. 1.



with the rail would be found to occur in a helical path on the surface of the circumference of the wheel. But if, as in practice, the wheel were compelled to move with the carriage, so as to follow the curve, then the wheel would be continually dragged endways (*i. e.*, in the direction of its axle) over the rail, to the extent of the endway movement it would have if left free, and the power expended in overcoming the resistance due to this cause would, therefore, be something very considerable, as it would be represented by the total weight on the wheels, multiplied by the coefficient of friction and by the amount of the endway movement (the resistance was shown in the model by a weight). It must be remembered that in this instance both wheels of a pair had to be taken into account, as both were moved endways over the rail. Moreover, the resistance arising from the endway motion of the wheel over the rails would, of course, increase as the wheel base of the engine was lengthened, because the farther the axles were from each other, the more would they deviate from a radius of the curve.

In both these respects, this species of resistance was unlike that arising from the slip due to the different lengths of the inner and outer rails in which, as he had before said, the weight on one wheel only could be taken with the total slip, and on which the length of the wheel base had no influence.

In reference to this question of resistance on curves, the Author of the Paper said, it was found that the engines under consideration gained speed on the curves instead of losing it. Looking at the construction of these engines, Mr. Bramwell was not surprised at this, when he heard that the steepness of the gradients had been diminished at the curves, as no doubt this diminution had been such as would have been suitable to an ordinary engine; and the Mont Cenis engines would not require so much allowance in this respect as ordinary engines, for the following reasons:—In the first place, the wheel base was very short—not more than 6 feet, or 6 feet 6 inches; therefore the deviation of the axles from radial lines was but slight, and the resistance he had pointed out as arising from this was consequently small. In the next place, as these engines did not depend principally on their weight to make adhesion on the rails, there was opportunity to make them as light as possible without diminishing their tractive force, and hence the weight on the bearing-wheels was comparatively small, when considered in reference to the power of the engine, and with a light weight the endway movement of the wheels over the rails, and the slip necessary to allow for the different lengths of the inner and outer rails was of little moment; moreover, the gauge was narrow, which rendered the slip due to the different lengths of the inner and outer rails of no great extent. The wheels with vertical axles that bore against the central rail, and gave thereby the large amount of tractive force, were of course unaffected by either of the causes of resistance on a curve.

These considerations would tend to explain why an engine on Mr. Fell's construction would not be affected in going round curves to the same extent as an ordinary engine. A remark had been made that, in the case of these engines, the correction by the conical form of the wheel could not apply, because the central rail prevented any sideway motion of the engine upon the bearing-rails. He thought, so far from not applying, it would apply with greater force and certainty, because, if the so-called central rail were (on a curve) set a little out of the centre, it would insure that the large part of the cone in the outer wheel bore on the rail, and that the small part bore in the inner wheel, and thus the advantage afforded by the cone would be rendered definite and certain, and not be left to the vagaries of the engine.

Mr. Fell stated that he had experienced some difficulty with the sledge breaks, and proposed to apply breaks to the wheels. Mr. Bramwell could not see how that would effect an improvement; the resistance must ultimately come upon the rail, which therefore must be worn; and if breaks were put on the wheels, two vital parts would be worn instead of only one. It would be better, in his opinion, to wear out the rails only by a distributed wear, rather than to wear first of all the wheels into irregular shapes, and then to wear out the rails with the action of these imperfect wheels.

On the subject of breaks, he would refer to a matter which he had heard from French Engineers in Paris, with respect to a system of breaking now in operation in trains in France working on inclines, viz., by reversing the engines, and without using the ordinary break contrivances, or using them only to a limited extent. One reason, he believed, why engines were not ordinarily kept in reversed gear in descending inclines was, that the cylinders became so heated that there was danger of the pistons cutting. The French Engineers had met that difficulty in this simple manner: they provided, when the engines were working in reversed gear, an inlet of steam, and also a small inlet of water into the exhaust; as the locomotive came down an incline, it worked the pistons, converting them into compressing pumps, which drew into the cylinders the steam and water from the exhausts, and the regulator being of course open, pumped them into the boiler; the heat in the cylinders was used in vaporising the small quantity of water thus admitted; and in this way, while going down an incline in reversed gear, the cylinders were really generating steam, which was conveyed into the boiler.

The Author of the Paper had alluded to the Blenkinsop rail. Mr. Bramwell had succeeded in obtaining a specimen of an actual rail, which he exhibited, and he had bought a copy of the specification of Blenkinsop's patent, dated 1811. This rail was made in accordance with the specification, which stated :—

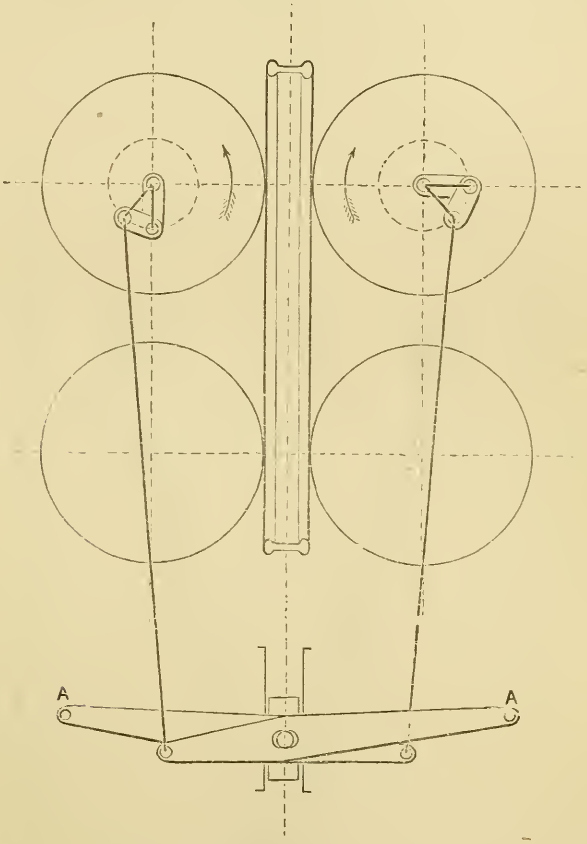
> "By preference, I do cast one of the sides or range of pieces forming the said railroad with teeth or protuberances, or other parts of the nature of teeth, standing as aforesaid, so that the same side or range shall constitute my said toothed rack or longitudinal piece, and at the same time afford a regular and even bearing for the wheels, and for the toothed wheel, which (if its plane be vertical) may be made with a side rim to bear upon the smooth part of the rail, and prevent the teeth from locking too deep."

As a historical fact, the Blenkinsop rail was undoubtedly very interesting, and it offered the advantage of affording a means of utilizing the power of an engine for traction without increasing its weight. But the Patentee appeared to have overlooked the objection, that the path of the smooth wheel was not coincident with the pitch line of the rack; and therefore, as the rack determined the rate of progression, there would be a certain amount of slip between the smooth wheel and the rail, as the diameter of the smooth part would differ from that of the pitch line of the toothed part. No doubt the remedy for this was simple, viz., leaving the bearing-wheel loose on the axle. In the Mont Cenis engines, the same end, of increasing the tractive force without adding to the weight, was aimed at, and they were free from the objection he had just mentioned, and from other objections connected with the Blenkinsop rail.

He apprehended that any system which was, as compared with an ordinary locomotive, equally efficacious for traction, but was unaccompanied by the manifest defect of increasing the weight for the sake of adhesion, deserved attention. There was nothing more shocking than the idea of adding to the weight of the machine, not to get the requisite power, but to get the requisite adhesion. To add to the weight of an engine, in order that a small portion of that weight might be available in getting up an incline, was clearly most objectionable; and he thought any one who helped to get rid of that stigma upon mechanical engineering deserved the thanks of the profession.

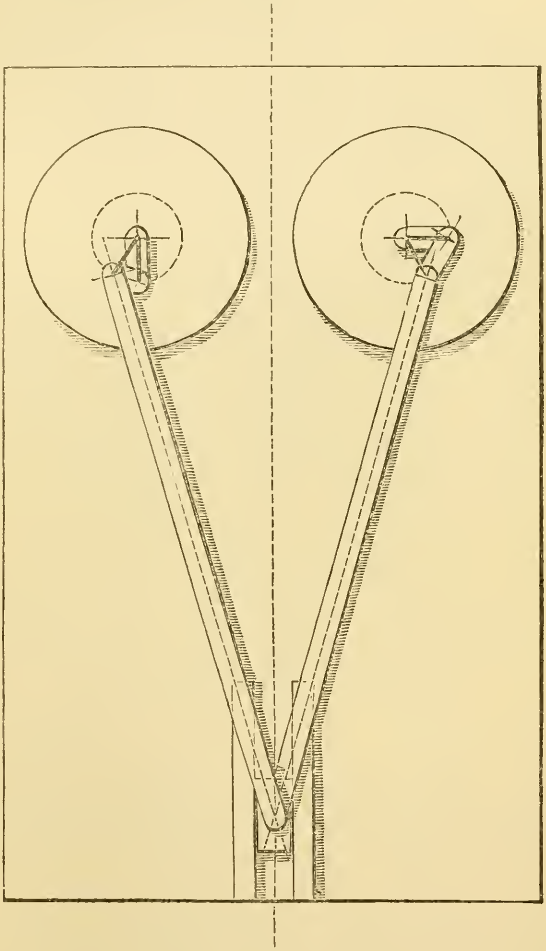
It would be remembered that Mr. Fell and Mr. Alexander, when speaking of the engine to be used on the Mont Cenis Railway, both alluded to the contrivance for getting the cranks of the vertical axles over the centres. He found, on examining the model, that this engine had two cylinders, working two pairs of driving-wheels on the ordinary rails, and also two pairs of driving-wheels on a central horizontal rail; for these two latter pairs the ordinary method of coupling the engines together was not applicable, as the vertical axles on one side would not serve as the means of coupling both engines, and therefore some fresh expedient must be resorted to. The method that was employed was shown in the model he had referred to, as well as in Fig. 2. In Fig. 2, the piston rods and ordinary connecting rods were not shown, but only the cranks of which they took hold. From these cranks, return cranks were made, in such position as to bring their two crank pins so as to be similarly situated on each side of the rail. From these latter crank pins, connecting rods, shown by lines in Fig. 2, were led to a pair of levers having their fulcra at A A, and placed near the front of the engine, which levers were connected by a common pin, so that one could not be moved without the other. It would be seen that when either one of the piston cranks was on the centre, the lever cranks were at an angle of 45°; and thus the piston that was not on the centre could, by means of the levers, work the other piston so as to get its crank over the centre.

Fig. 2.



It had been stated that, when it was made, this contrivance was not liked, and that some substitute would have been acceptable. It was possible that what he was about to propose might have been thought of before, and though, in a mechanical point of view, it might be regarded as not strictly orthodox, yet he believed it would answer the purpose, and would be more simple. It would be seen in the Mont Cenis engine that, by the arrangement of levers, it was practicable to bring the two connecting rods that joined the ends of the levers to the cranks into such position that their average centre line should be parallel to the centre of the engine; but if, as he would suggest, this position of parallelism of the connecting rods were slightly varied, they might be brought together to a central block, and the levers be entirely dispensed with.

Fig. 3.



Without, however, interfering with the efficiency of the apparatus in getting the engines over the centres, the cranks for these connecting rods would of course require their position to be slightly altered, to suit the different average centre line of the rods.

Mr. Pole had cautioned Engineers against the use of steel. No doubt, of all metals, steel was one which varied most in quality, as there might be one quality of steel which would melt with a low heat in an hour, while another quality would take four hours in a Siemens' furnace to melt it at all. Two such materials (both steel) differed, of course, very widely; but he thought it was wrong, on account of such variation in the same metal, to deprecate altogether the use of steel. Provision should rather be made against the uncertainty of the quality, by endeavouring to bring the steel trade into the same state of uniformity of excellence as had been attained in the Yorkshire iron trade, and thus to make sure that the same quality of steel should always be had for a given brand. Mr. Ramsbottom (M. Inst. C.E.) had stated that he had experimented with steel tires, bored out, afterwards made red-hot, and in that state placed upon solid cast-iron centres, and then left to cool; and this without a single case of fracture of the steel. He had also stated that since he had used steel axles, the fracture of a straight axle was unknown. Mr. Bramwell thought these were evidences in favour of the use of steel. In his opinion, the end to be aimed at was to be able to rely on obtaining a proper and uniform quality of steel; and he thought it would be highly impolitic to discard the use of steel altogether, merely because some steel was bad.

Mr. Margary would remark incidentally, on the subject of steel, that it was to be feared that Engineers sometimes endeavoured to beat down contractors' prices, and that contractors, in their turn, endeavoured to pull down the manufacturers' prices, and a bad quality of metal was afterwards the result. On the subject of the atmospheric system he would remark, that having been Mr. Brunel's assistant in the South Devon atmospheric experiments, he might be able to answer Mr. Hemans' observations with regard to the effects of frost upon the valve. Frost did affect the valve to some extent. It might be said that on the south coast of Devon there was seldom frost of long duration; but there had been 16° of frost there this year, and when the atmospheric system was in operation there had been as much as 12°, but it was obviated by the substitution of a different grease from that used in summer. The great drawback was the difficulty at all seasons of sealing the valve at the commencement of pumping; but Mr. Brunel considered that might be overcome. It was only due to the memory of Mr. Brunel to say, that he conscientiously backed his opinions, with regard to the atmospheric system, with his own purse. If he had had the national purse to resort to in the case of experiments on this system, as was now the case with experiments on guns, &c., no doubt Mr. Brunel would have carried out the atmospheric system to a success; but the shareholders pulled their purse-strings at the moment he thought he was about to bring the system to perfection. The atmospheric system led to steep inclines being adopted on the South Devon Railway, over which the trains were now worked with locomotives. The curves varied from 15 to 20 chains radius.

The goods engines, having cylinders 17 inches in diameter, with a length of stroke of 24 inches, and six coupled wheels 4 feet 9 inches in diameter, took up the Dainton incline a load of 180 tons at a speed of about 10 miles an hour, the weight of the engine being 38 tons. The passenger engines having cylinders 17 inches in diameter, with a length of stroke of 24 inches, and four coupled driving-wheels 5 feet 9 inches in diameter, with bogie wheels 3 feet 6 inches in diameter, took seven loaded carriages, making a total load of about 90 tons, up the same incline at a speed of about 10 miles an hour. He hoped that other Engineers would state what was done on the inclines under their control. There were a great many inclines which ought to be mentioned, as, for instance, that at Bromsgrove, and that on the South Western Railway between the two Exeter stations, where two engines were frequently employed to take up the trains; and he should be glad to be informed what was their ruling load. He had received, from Mr. David Murray, some particulars with respect to the contractor's incline at the North Eastern Defences at Plymouth. The tank engine used on this work was built by Messrs. Fletcher and Jennings, of Whitehaven. It had four coupled wheels 4 feet 6 inches in diameter, the cylinders were 11 inches in diameter, the tank was underneath the boiler, and the total weight of the engine was 18 tons; the gauge of the line being 4 feet 8½ inches. This engine took up the Woodlands incline of 600 yards, with a gradient of 1 in 27½, 70 tons dead weight at an average speed of 10 miles per hour; the maximum steam pressure was 110 lbs. to the inch, reduced in running to 95 lbs. and 90 lbs. The Knackers Knowle Bank was on a gradient of 1 in 22, for a length of 400 yards; with steam as above the engine took up 50 tons dead weight at the same average speed.

Perhaps Captain Tyler would say, whether the sledge breaks acted upon the middle rail or on the running rail; because if they acted upon the running rail, he could state from his own experience that they would be most dangerous. The tank engines introduced by Mr. Brunel on the South Devon line had sledge breaks at first, which constantly went on the wrong side of the crossings and greatly damaged the rails generally. They also considerably reduced the weight on the engine wheels.

Captain Tyler said the sledge break was on the centre rail.

Mr. Margary added, that he would have been glad to have heard Mr. Fell's opinion as to the economy of his system for branch railways, and whether he recommended it in the case of short lines, as it would require different working plant from that on the main line. As the load that could be taken in a train was small, it appeared to him there would be much delay in dividing trains, which a line of less steep gradient would obviate. He would remark incidentally, that good ballast should always be put on inclines.

Mr. Alexander Doull described, by means of a diagram, a system of atmospheric railway, which he suggested as applicable to the working of steep gradients.[1] The motive power consisted in a hollow revolving drum, fitted with four blades, at the mouth of the tunnel, by means of which air was either forced into or extracted from the tunnel.

Mr. Fairlie said, Mr. Fell's engine was of special design, and had attracted a good deal of attention. Those who were acquainted with the subject of locomotive construction would admit that in a first design it was almost impossible to arrive at perfection; such no doubt had been Mr. Fell's experience, as it had been his. With reference to his engine, the question of draught in the fire-box, selected as one of the main objections by Mr. Conybeare, was considered when he gave the specification of the engine to the manufacturer; and it was decided that putting a water partition across the fire-box would be a disadvantage instead of an advantage. The objection to the engines now working on the Neath and Brecon line was, that there was no water partition across the fire-box, and that the draught from one engine counteracted the useful effect of the other, by drawing the cold air down the chimney into the fire-box. Since he had put a brick partition across the fire-box of one of these engines it was found that the draught was perfect. One of these engines, with four cylinders each 10 inches in diameter, and having a length of stroke of 16 inches, was working on a line having inclines varying from 1 in 50, with curves of 10 chains radius, where there was not a piece of straight line of more than half a mile. It took daily a load of 100 tons, exclusive of its own weight, for 14 miles, with a mean ascent of 1,250 feet. With regard to the steam pipes, it was considered at first sufficient to put in a single coil pipe, one end being attached to the boiler in the usual way and the other to the cylinders. This was found not to possess sufficient elasticity, and gave way when working a curve of 2½ chains radius on a gradient of 1 in 30; but he subsequently put in a pendulum steam pipe which had worked perfectly to the present day. He was sorry the locomotive superintendent of the line, on which these engines were employed, was not present to give some further facts with regard to their working. The double boiler duplex bogie engine, a model of which he exhibited, had four cylinders 18 inches in diameter, with a length of stroke of 24 inches, and wheels 4 feet in diameter. The wheels were disposed in groups of six coupled together, the whole engine weighing 72 tons, divided equally over the twelve wheels, or 6 tons on each. The water was carried in tanks, made in the form of box girders, placed horizontally the whole length of the engine on each side, and capable of containing 2,600 gallons. These tanks formed a strong framing, running the entire length of the engine, and supported the boiler. They were tied together by transverse frames under the boiler, which had attached to them what were termed the bogie pins. The strains of tension or compression were transmitted from one bogie pin to the other through the box girder tanks, without affecting or communicating with the boiler in any way whatever; thus the two bogie engines were made one, while they were at liberty to swivel or conform to any curve they might happen to be on. The fire-box was in the centre of the boiler, with two sets of tubes running in opposite directions, and there were three transverse water partitions to make the draught perfect. It was fed from the top, through a large steam dome, by four fire-hole doors, from which every part of the fire was accessible. The heat from the box acted as a superheater to the steam collected in the dome; the coal was disposed on each side of this dome, or by fire-holes in the sides, as now in use. The engine-man and stoker were housed under a hood, while the foot-plate on which they stood was common to both fire-boxes, a clear passage being open at all times from side to side of the engine. The engine could be started, stopped, or reversed at either side, and the breaks applied in the same manner. It was perfectly under the control of one driver and one fireman, although capable of exerting twice the power of the most powerful ordinary engines.

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[1] *Vide* "The Mechanics' Magazine," vol. xvi. (New Series), p. 55.

The single boiler duplex bogie engine was a great improvement over the engine with a tender, inasmuch as in the former everything carried was made available for adhesion. It differed from Mr. Sturrock's arrangement, inasmuch as the boiler, and the water and fuel bunkers were made part and parcel of each, by means of a strong framing running the entire length of the engine, with the bogie pins fastened thereto in the manner before described. This framing formed a cradle on which the boiler was placed. There were six coupled wheels forming the group on the bogie frame under the barrel of the boiler, driven by two cylinders of 18 inches diameter, with a length of stroke of 24 inches. The group under the water and fuel bunkers was composed in this case of four wheels, but six might also be used in a bogie frame. These bogies allowed any curve or reverse curve to be followed; and the centre line of the boiler frames, &c., formed the chord of an arc described by the bogies when placed on a curve. Precisely the same position was taken in the case of either engine, and it had this advantage, that the centre of gravity of the boiler and its accessories forming the chord passed nearer to the centre of the radius of any curve, by a distance equal to the versed sine, thus to a large extent overcoming the centrifugal force when the engine passed round curves at considerable velocities. The ordinary type of engine when requiring the most trifling repairs had to be laid up, and another put in its place. This was not so with his system, because the engine was divided into four distinct parts, the boiler, the carrier frame, and the two bogie frames, which, while forming a whole, were complete and distinct within themselves, so that should one of the latter fail or require repairs, it was only necessary to unfasten six bolts and the steam pipe connections, remove the bogie and substitute another, all of which could be done without lowering the steam. This was a matter of grave importance, particularly in countries where skilled labour was difficult to be had, as described by Mr. Lloyd. His own experience, in the construction of a railway in Venezuela was, that skilled labour was the greatest difficulty he had to contend with, especially that of Englishmen, who seemed to forget their proper position the moment they left their own country. It was he believed a fact, that on the Great Western and London and the North Western Railways every engine at work was laid up for about two months out of each year for repairs. He claimed for his engine that the arrangement was simplicity itself, and he did not hesitate to assert, that it was impossible for any two engines coupled together in the ordinary way to do an equal amount of duty. The measure of a curve round which an engine would travel was the length of its wheel base. In the ordinary construction the wheels could not be placed so close together as in his engine, because the weight of the engine would be unequally distributed on the wheels. The effect on the permanent way of placing all the wheels under the barrel of the boiler was well known; each end overhanging the wheels, especially the fire-box and foot-plate, could be best likened to a loaded lever, which, when forced up and down by the oscillation of the engine and by the momentum, destroyed the rails. It was a well-known fact to those acquainted with the subject, that it was not the normal load upon the wheels of the locomotive which caused the destruction of the permanent way, but it was the extra weight of the superstructure caused by the momentum which gave the blows, as before described; this weight being borne on four, six, or eight points, went bounding from one point to the other, and it was these blows through the wheels, indicated by the rise and fall of the boiler, &c., on the springs, which destroyed the rails. He had hoped to have been able to bring forward the subject of his locomotives, with the statistics of their performances, in a more perfect manner, but he regretted he could not do so at present.

With respect to the performance of the engines of Mr. Fell and Mr. Woods, it would seem that, to carry a load of 77 tons, Mr. Woods required an engine weighing 57 tons, only 24 of which were effective for adhesion. The average load on the Chili Railway was only 57 tons, and the exceptional load 77 tons. Mr.

Woods' engine had only about 38 per cent. of the total weight effective for adhesion, while, to contrast with this, Mr. Fell had, it was said, 200 per cent. effective adhesion—a difference which required explanation. He believed that an engine on his principle, weighing only 50 tons, with twelve wheels, or four less than in the Copiapo engine, all available for traction, would draw double the load up the same incline.

Mr. Abernethy thought justice had scarcely been done to Mr. Brunlees in bringing the Mont Cenis Railway forward at this juncture, because there were many difficulties to be overcome, which no doubt would be successfully surmounted. His attention was called to the Mont Cenis Railway some years ago, when he was constructing works in Italy. As to the possibility of engines working over the gradients of the Mont Cenis line, he was convinced that the mere question of traction had been completely solved. But the difficulties to be overcome were these:—There was a great depth of snow over Mont Cenis during the winter months. He understood it was the intention of the Engineer of this railway to construct covered ways, partly of wood and partly of a more solid character, at points where the road was exposed to avalanches; but he was convinced from his experience—having often crossed Mont Cenis during winter, and having been on one occasion detained on the summit thirty-six hours—that it would be necessary in regard to all Alpine passes, beyond a certain elevation, to form continuous covered ways, and that not of a slight character, as a provision against the great depth of snow, apart from the question of avalanches. He had recently crossed Mont Cenis, and could see but little of the railway from the great depth of snow. It then occurred to him, that neither by snow-ploughs nor by any other means, would it be possible to clear away and overcome a depth of from 10 feet to 15 feet of snow. The length of covered way required affected materially the question of economy of construction, as between a tunnel through the mountain and covered ways over the summit or snowy region. He thought the covered ways should begin at an elevation of about 5,000 feet above the level of the sea, and be provided continuously above that level. He repeated his conviction that, if this system was to be adopted, in mountain passes of this description above a certain elevation, there must be continuous covered ways; and, at those parts which were exposed to avalanches, tunnels of a thoroughly substantial character. He was, moreover, of opinion that the railway should be laid, as far as possible, on the inside of the public road, and not upon the outer edge of it, because, after the melting of great masses of snow, the outer edge was apt to be disturbed. From Lanslebourg to St. Michel the road was exposed to the action of violent torrents, and before the end of the winter a good deal of it might be destroyed.

It was advisable, therefore, in executing a railway of this character, to make it as independent as possible of the ordinary road. He had no doubt of the success of this railway as respected the question of traction simply.

Mr. J. A. Longridge said, the subject of the Paper was not upon the construction of the Mont Cenis or any other railway through the Alps, but upon the best way of working steep gradients, supposing they were absolutely necessary; and on that subject he would give the results of his experience. He had laid out a railway in the Mauritius some years ago, which was subsequently executed; and, though not worked by himself for passenger traffic, he had worked it for ballasting and for the transport of materials. The ruling gradient of that line was 1 in 27. The ballast-trains consisted of twenty wagons in fine weather, with an engine at the front and back, which he believed was a safe way of working these heavy gradients, because it rendered them, to a great extent, independent of the breaks. Each of these engines weighed 37 tons, and each truck-load weighed 13½ tons, making a total load of 270 tons, besides the two engines weighing 74 tons. It was found that in fine weather, when the rails were dry, the inclines of 1 in 27 could be ascended, with wheels 3 feet 9 inches in diameter, at about 8 miles an hour; but when the rails were wet from rain or dew, the load had to be reduced by four, or six, or more trucks: the coefficient of friction, under these circumstances, came out as one-fifth. There could not be a doubt that it might be worked with a coefficient of from one-fourth to one-fifth; but it was not at all desirable that such a coefficient should be depended upon, because, on a dewy or damp morning, the train might come to a standstill. He believed, in any extreme gradient, particularly such as had to be dealt with in Alpine passes, the central rail system was the only one that could be depended upon. At the same time he did not entirely like the engine Mr. Fell had adopted; it was very complicated. He preferred to have two pairs of cylinders, one for working the horizontal, and the other for the vertical wheels; but further experience would settle that question. If he required an engine to work over long gradients of 1 in 12, or 1 in 14, he would depend entirely upon the horizontal wheels, and would make the whole of the other wheels simply bearing wheels, each wheel revolving on its own axle. He believed there would be an immense gain of tractive force if the wheels were not fixed to the axles. He had had lines under his notice on which, as a rule, the gradients were generally good, but with here and there a steep gradient for a distance of half or three-quarters of a mile; and where this had to be got over with a heavy load, there must be an engine sufficiently powerful to draw it. Under such circumstances, he thought it worthy of consideration, whether it would not be best to have an eight-wheeled engine, having four driving-wheels, with a balance lever supported near the centre of gravity of the engine, and provided with a lever arrangement by which the springs could be screwed down when the engine was running, so as to throw the whole weight upon the driving-wheels when going up a gradient, and, when running on a level, distribute the weight over the whole of the eight wheels equally. Such an arrangement would obviate the use of sand in going up an incline; it would be simply necessary to screw down the springs, and throw the weight upon those springs while going up the gradient. In that case the steep gradients might be laid with heavy rails, and the light gradients with light rails. In this proposed engine, the axles of the driving-wheels would be placed within 5 or 6 feet of each other, while the leading and trailing axles would be fitted with Adams' radial axle-boxes; and such an engine could move with ease round curves of very small radii.

With respect to steel, he had been informed by a gentleman who had been for many years at the head of the working department of the Belgian railways, that his late experience with regard to steel was such, that he recommended the Minister of Public Works to go back to iron entirely. A great price had been paid for steel rails, but they were so inferior, owing to the competition amongst the manufacturers, that they could not be depended upon.

Mr. W. I. Ellis observed, with reference to the more prominent subjects of the Paper, that it seemed necessary to point out that Mr. Fell's engine did not supply any recognized deficiency in the locomotives hitherto used on steep gradients in this country; it being, rather, a very creditable and ingenious adaptation of the locomotive to a particular purpose, in which the speed of the engine had been reduced to augment its tractive force; whereas it had been found that the weight of an engine, and more particularly of a tank engine, gave sufficient adhesion to resist the tendency to slip at speeds considered necessary on ordinary lines. In confirmation of this he might remark, that, the large engines used upon the Vale of Neath Railway in 1856 and 1857 were found to take as heavy a load, after being altered from tank engines into engines with a tender, as before the alteration. These engines weighed upwards of 50 tons, not equally distributed on the six wheels, and the alteration was made after the Company found that this excessive weight had caused great destruction to the permanent way. This also was altered from a longitudinal sleeper road with light bridge rails, into one with transverse sleepers, chairs, and heavy double-headed rails. The presumption was, that a weak permanent way was quite out of place on steep gradients and sharp curves, the rails being rapidly worn by slipping and by the action of the breaks. This wear might be aggravated when there was an excessive weight on any of the wheels of the engines, but must in all cases be great where the traffic was heavy. He was of opinion that the English were much behind the French in the construction of engines for steep gradients. Some Engineers were reproducing designs which had been tried and abandoned on the Continent. By reducing the diameter of the engine-wheels to 3 feet 6 inches, the French had been enabled to reduce the height of the frames, on the top of which they had placed the boiler, instead of between them; and being thus able to extend it laterally over the wheels, they were practically independent of the width of the gauge of the railway, and had constructed engines with boilers of great size. The following were the principal dimensions of some tank engines, built by Gouin and Cie, for the Northern Railway of France in 1863. The engines were each on twelve wheels, arranged in one frame, with four outside cylinders, the wheels being coupled together, as in two separate six-wheel coupled engines. The diameter of the wheels was 3 feet 6 inches; the diameters of the cylinders, and the length of the stroke, were each 17$\frac{5}{16}$ inches. The area of the fire-grate was 35 square feet, and of the heating surface 2,200 square feet. The length of the wheel base was 19 feet 8 inches. With this dimension it would be impossible to pass easily round sharp curves. There were also defects in the boiler which would cause a liability to prime, though it had been attempted to remedy this by the use of a large super-heater. There was also a deficiency of break power; the break blocks being limited, for an engine weighing about 67$\frac{1}{2}$ tons, to four break blocks acting on the middle wheels, though twice as many could be used by applying them to the leading and trailing wheels. The conditions of economy in the steam-engine required that the functional parts should be as few and as large as possible; this was well carried out in the engine just described, but was lost sight of in some of the designs, lately put forward, of boilers with duplicate fire-boxes or barrels. In 1858 the Vulcan Foundry Company constructed for Mr. Cubitt some tank engines, with a wheel base of only 10 feet 5 inches, in which the diameter of the wheels was 4 feet, the diameter of the cylinders 16 inches, with a length of stroke of 24 inches, and the weight about 30 tons. The unusual shortness of the wheel base enabled these engines to travel with great ease round sharp curves on the Rhymney Railway, and on some other lines in the same neighbourhood, the Directors of which had been induced to adopt them by their success on that line. In 1864 two engines were built by the same firm for the Vale of Neath Railway, for which he had designed the boiler. The rings of the barrel were made of one plate, each jointed near the top of the boiler, and the longitudinal seams were designed to be covered by strips, or plates, riveted on, each ring breaking joint with the one next to it; but he was not aware whether these joint plates were applied exactly as designed or not. The boilers worked at 150 lbs. pressure per square inch, and had fire-grates of $18\frac{3}{4}$ square feet area; a fire-box with a transverse midfeather; two hundred and fifty-one tubes, 2 inches outside diameter, and a total heating surface of 154 square feet; the cylinders were 18 inches in diameter, with a length of stroke of 2 feet; and with a 1,200 gallon tank, the engines were estimated to weigh 43 tons. Two other engines were built at the same time by Messrs. Slaughter, in which the same plan of boiler was adopted. The copper fire-box was put into the casing either from the top or the front, before the casing plate was riveted in, by which means the fire-box could be widened out at the sides, so as to overhang the wheels, when they did not exceed 5 feet in diameter; and the usual number of tubes, about one hundred and eighty for a narrow-gauge engine, could be raised to two hundred and fifty-one, or even more, increasing the boiler power in proportion. The economical advantages of powerful engines were so great that, he believed, there was a desire to increase the power of the goods engines on the principal lines in the kingdom, and this appeared to him the most available plan for the purpose. The weight was divided as equally as possible on the six wheels, with a wheel-base of 15 feet 6 inches; and on these and the Rhymney engines the break was made to act on all the wheels. Messrs. Slaughter's engines had eight coupled wheels, with a wheel-base of about 14 feet 3 inches; and two similar engines, ordered, he believed, for goods traffic on the Metropolitan Railway, were at work in the goods station at King's Cross. As to adhesion, both the Vale of Neath and the Rhymney engines worked on a gradient of 1 in 47; and the load conveyed was equal to four and a half times the weight of the engine, beside the engine's own weight; and this load would correspond with an adhesion of one-eighth. He had received a letter from Mr. Bishop, the Locomotive Superintendent of the Vale of Neath Railway, in which it was said of the engines that weighed about 43 tons, "I never tested them as to the exact weight they would take up the Glyn Neath Incline (gradient 1 in 47 for 5 miles), but the ordinary load I allot them, when the traffic requires it, is 190 tons, net. This they take up at the rate of 7 to 8 miles an hour: with one of the eight-wheel coupled engines, the weight of which is 56 tons, when in trim to start with a train, I have taken up the same incline 300 tons, at the rate of $6\frac{1}{2}$ miles an hour. The ordinary load allotted to these is 230 tons. The adhesion in all these cases, with the exception of the 300-ton load with the eight-wheeled engines, is nearly one-sixth." The limit of weight on each pair of wheels (14 tons) having been reached in the Vale of Neath engines, and the length of the wheel base not allowing more wheels to be coupled together, and the limit of the power of the engine being its weight, to obtain more powerful engines the frame or carriage must be a jointed one, if a tank engine be employed; or the tender wheels must be made available for adhesion, if a tender be used. This latter had not yet been done advantageously, but by the use of four cylinders taking steam from one large boiler, a tank engine might be made capable of passing round any main line curve, and possibly with a break acting on all its wheels, the relative economy of the engine increasing with its size. Continental Engineers had been beforehand in appreciating the true condition of a carriage passing round a curve. In this country it had been generally taken for granted, that an engine was impeded on a curve by the leading and trailing wheels on one side rubbing against the outer rail, and the driving-wheels on the other side against the inner rail; the fact being, as might easily be imagined, from the tendency of the wheels to run in a straight line, that one of the leading wheels was brought into contact with the outside rail, and the opposite trailing-wheel with the inside one; the middle wheels occupying an intermediate position, not touching the rail with the wheel flange on either side, thus showing the inutility of taking the flanges off the middle wheels as had been so frequently done. Great reliance had been placed on the coning of the wheels, for ease in passing round curves; but it was evident that this operated prejudicially in trailing-wheels, and was so slight, and the play between the wheels so small, that it was of little benefit in the leading wheels, being insufficient at the best, except when the tires were newly turned on small wheels running on curves of large radius. The gauge of rails on a curve should be widened until the play of the wheels between them was equal to twice the versed sine of the arc of which the longest wheel base was the chord; if not, extra resistance would be caused by the vehicle being jammed between the rails. Having been in the habit of timing the passage of trains round curves for some years, more particularly on one of 10 chains radius, he soon found that all trains were reduced very nearly to one uniform speed of about 15 miles an hour on that curve, and as (beyond these observations) he thought it would be admitted that a train might as safely run round a curve of half a mile radius at 60 miles an hour, as round a 10-chain curve at 15 miles an hour, it followed that a complete circle would in all cases be described in just the same time, irrespective of the length of the radius, provided the highest speed was in the capability of the engine. It further followed that, as the centrifugal force of a body varied as the square of the velocity and inversely as the radius or as the angular velocity, the greatest cant or elevation of the outside rail should be on curves of the longest radii; except that the cant might also be useful on sharp curves in throwing the engine in the direction the leading wheels should take. He believed that more accidents from running off had happened to express trains on curves of large radii than on sharp ones, which might probably be thus accounted for. On the same 10-chain curve he had noticed circumstances which had led him to conclude, that the resistance to the passage of an ordinary six-wheeled engine was as great as on a gradient of 1 in 85. It was greatly to be desired that experiments to determine the resistances on curves should be undertaken; but he thought it would be found, that a six-wheeled coupled engine created double the resistance of an uncoupled one, and that in different carriages on the same curve the resistance would be in proportion to the square of the wheel base, and on curves of different radii inversely as the radius. This almost necessarily followed from the fact that, within any limits that could possibly occur on railway curves, the versed sine of an arc of a curve varied as the square of the chord and inversely as the radius; so that a reduction of the wheel base was the most effective method of diminishing the resistance. The minimum radius of curve round which a carriage could be run was proportional to the wheel base and the gauge of the rails, and should not be less than nine times the product of these two, if speed was any consideration, or the factor might be reduced to four times, if a slow speed were used; but in this case the resistance and the wear of the wheel flanges would be excessive. The means that had been adopted to facilitate the passage of long carriages round curves were all governed by the idea of placing some of the axles in a swivelling truck, or frame, so that they could take a radial direction on the curve—the system of having loose wheels having been abandoned on trial. Of these, the original plan was to have a bogie turning round a fixed centre; but the more modern plan of placing the real or supposed centre of swivelling much further back than the centre of the truck, as in Bissell's bogie and the radial axle boxes of Mr. Adams, was far superior, from the action being the same as if the carriage were divided into two separate and independent ones, and he mentioned this to point out that there was a position in which this centre might be placed when the bogie or radiating axles would take a radial direction, and the longitudinal centre line of the truck be a tangent to a curve of any radius whatever.

Mr. E. A. Cowper said, Mr. Woods had spoken of the adhesion of the wheels being sometimes as much as one-fourth on dry clean rails, and as the fact appeared to be doubted, whilst he knew it to be the case, Mr. Cowper had made a rough model with a stout iron wire, firmly bearing on a strong piece of wood to represent the rail, with a heavy weight bearing on a small surface on the rail, to represent the wheel.

The amount of adhesion of metal upon metal depended very much on the presence of a lubricator, but also much more than was commonly taken into account, on the weight per square inch on the surfaces; for whilst surfaces such as 'bearings' would work well with about 700 lbs. per square inch, or even a little more, they were certain to squeeze out the lubricator, and cut, if anything like 10 tons per square inch came on them, as the two metals then came into absolute contact. The inclination at which the model was then fixed was 1 in 4, and the wire had been 'drawfiled' in the direction of its length, in order to have it perfectly dry and clean, and as nearly as possible in a like condition to the rails, say at the entrance to a station—the weight stood perfectly at that inclination of 1 in 4; and with great care he had made it stand at 1 in 3.

The rail was represented by a piece of wire $\frac{3}{16}$ths of an inch in diameter, and the weight, composed of two railway chairs, rested upon a piece of wire on the wire rail, and so, to some extent, represented the condition of a line of railway, though the weight was far less per square inch than in the case of a railway wheel having 7 tons on it. The ordinary rail was rounded to a certain extent on the surface, while the tire of the wheel was rounded in the other direction; the two curved surfaces coming into contact, mathematically speaking, only touched at a point; but it was well known that the ultimate crushing resistance of iron was such that it could not by any possibility bear the whole of that weight on a point. However, supposing the place of contact was something like a quarter of an inch square, or one-sixteenth of a square inch, and that there were 7 tons on the wheel, that would amount to 112 tons per square inch, and with such a weight, without lubrication, the surfaces would cut or tear if there was any motion between them. So in the case of the model, the pieces of wire touched only on a very small surface, and the weight of the two chairs, giving probably about 10 tons per square inch, was sufficient to make the surface cut, and enabled them to stand at that steep inclination of 1 in 4—whether the surfaces were fresh, or whether the weight had been forcibly slid up and down the incline several times. This showed that an engine would go up an incline of 1 in 4. He thought the model was as nearly as possible in the condition of a rail when the wheels had been slipping. On a line where the wheels were commonly stopped and slipped on entering the stations, they acted as sledges on the rails, and they tore the surface of the rails. That was the condition of the metal on this model, as it had been smoothly drawfiled in the direction of its length; the wire was cutting more or less; but the least amount of grease made the weight slip down.

From the average of a large number of experiments, carefully conducted by different persons who had used the model for the purpose, it was found that with a dry and clean rail it required the utmost care to succeed in removing all grease, the rail not only requiring to be first wiped perfectly clean, but to be afterwards filed up with a fresh clean file free from all grease, or the experiment would be entirely fallacious. The results of the experiments were given in the following Table, the exact condition of the rail being stated in each case.

EXPERIMENTS ON ADHESION,

With a pressure of about 10 tons per square inch.

Surfaces burnished and greased with tallow	1 in 9½
Surfaces drawfiled and greased with tallow (after some wear) .	1 in 9
Surfaces fresh drawfiled and greased	1 in 8
Surfaces burnished and wetted with water	1 in 5
Surfaces burnished and dry	1 in 4
Surfaces drawfiled and dry	1 in 3

Mr. R. P. BRERETON remarked, that it had been assumed by several speakers, that adhesion to the extent of one-fourth was quite possible under certain conditions. It appeared to him that if such were the case no engines in use in this country would be able to avail themselves of it. He had made experiments on inclines on a broad-gauge line, in order to see what inclination could be surmounted, by one of the heavy broad-gauge goods engines that had been constantly taking a load of 200 tons up an incline of something over 1 in 40. The engine ascended 1 in 17½, 1 in 15, and 1 in 12, which he found was the limit; it invariably stuck fast beyond that gradient. This arose not from want of adhesion, but from want of power in the engine to do any more. At particular points when it stuck fast, he had no doubt there was an amount of adhesion of 1 in 8·8, but then there was only one cylinder at work and available to start with. He had never seen an instance of want of adhesion at that gradient, as the engine always worked its way up assisted by a rope: if the adhesion had been less he presumed the wheels would have skidded. Mr. Fell did this by some nipping apparatus with small radius of the wheel as compared with the cylinder and crank. Anything of nipping on a running rail would be very small indeed with engines in ordinary use in this country, and but small effect would be got from it. Much of the increased resistance, frequently met with at sharp curves, did not really arise from anything in the curves themselves, but was due to a considerable steepening of the gradient at the commencement of a curve, brought about by the raising of the outer rail. This was sometimes done on a broad-gauge line to the extent of 6, 8, or 10 inches, in a length of a chain or thereabouts, adding a gradient of 1 in 80 or 1 in 100 for one half the weight of the engine and train to be taken up, in addition to the general gradient of the railway. Assuming an engine to be just doing its work with a heavy train on a steep gradient, the above addition was severely felt, and might perhaps have been attributed entirely to resistance offered by the curve, particularly as it appeared to be so both at the commencement and at the termination of the curve in ascending as well as descending. In the former case, on entering the curve from the straight line, there was the direct increased resistance of the gradient, and on leaving the curve the diminished resistance due to a corresponding flattening. In the latter case, in descending, on entering the curve increased resistance was offered by the flattened gradient, and on leaving diminished resistance was felt upon the steepened portion. The pressure of steam in the case where the engine would not go up a gradient of 1 in 12 was 120 lbs. to the inch without any load; but that was not maintained long after starting.

Mr. Wm. Lyster Holt observed, through the Secretary, that the defects in the construction of Mr. Fairlie's engine had long since been remedied, and that instead of being complete failures they were a great success, and combined all the "good" qualities of bogie and twin engines without any of their bad points. It was true that at first the draught in the fire-box was baffled, and consequently steam could not be maintained on the banks with heavy loads; but this arose from the fire-box being constructed without a transverse water partition in the centre, as originally intended by the inventor. Since the fire-box had been divided transversely by a temporary brick partition the draught was perfect. There was an abundance of steam, and the result was so satisfactory, that these engines already burnt 10 per cent. less fuel than those of the ordinary type with equal loads over the same road. When the water partitions which were being made were completed, it was believed that a saving could be effected of at least 20 per cent. of fuel per H.P. exerted over the engines in general use. It was true also that one method of the many that could be applied for conveying steam from the smoke-box to the cylinders, failed from insufficient elasticity in the single coil copper pipe used for that purpose. But this occurred when the engine was working over a temporary road of 2½ chains radius on a gradient of 1 in 30. In an engine fitted with an improved construction of steam pipes, there had not been the slightest difficulty, and the swivelling motion of the bogies did not affect them in the least. This engine, having four cylinders 10 inches in diameter with a length of stroke of 16 inches, eight wheels 4 feet in diameter coupled in sets of four, and with the boiler pressure at 120 lbs. per square inch, constantly took loads of 100 tons up gradients of 1 in 50, having numerous S curves of small radius, while it had hauled as much as 130 tons over the same road. No other kind of engine would, he thought, under the same conditions, and with the same tractive force at the rails, draw an equal load. These engines possessed many advantages which rendered them especially fit for working on steep gradients and curves, as they passed round sharp curves with great facility, and with remarkable steadiness and perfect safety, even at high speeds. The wear of the permanent way and tires was reduced to a minimum. The whole weight was available for adhesion, and this weight was distributed equally over all the wheels. The peculiar form of the engine admitted of the use of a boiler of almost unlimited size, and therefore large quantities of steam could be generated. An increased tractive force could be obtained by four cylinders, and these afforded greater facilities for working expansively than two cylinders, and being smaller were more easily attached to the frames; while the power of the engine being divided into four, the strains and jerks would not be so violent on any particular part of the machine. Two men had perfect control over the engine, of which the motion was not more complicated than that of ordinary engines. It was very accessible for repairs, the bogies being easily removed from under the boiler barrels in a few minutes, and the expenditure of fuel was small.

Mr. Bramwell begged permission to offer a few words in explanation of what he had previously said. He was sorry to find he had unintentionally misquoted Mr. Naylor's observations. Mr. Naylor informed him that, in speaking of the increased retardation on curves caused by the slip due to the difference of length of the inner and the outer rail, he only took half the weight of the engine as being influenced by the slip, instead of the whole weight. He was under the impression that Mr. Naylor had taken the whole weight. So far, therefore, as his remarks on Mr. Naylor's observations referred to the whole weight having been taken with the slip, he wished those remarks to be considered as withdrawn.

Mr. Phipps called attention to a formula expressing the resistance due to curves generally. This he hoped would usefully supplement the particular demonstrations which Mr. Bramwell had given. He would endeavour to show, as briefly as possible, what were the circumstances attending the development of resistance upon curves, and in what manner this general formula was arrived at.

In the passage of a train round a curve there were, when the axles of the carriages were parallel and the wheels cylindrical, two principal sources of resistance. One, which might be called the radial resistance, arose from the tendency of the wheels to travel along the same straight line on which they were originally placed, which line being the chord of an arc of the curve, any progress along it would cause the wheels to leave the rails, and therefore also to be more remote from the centre of the curve. The relation of the above distance to the distance travelled forwards by the train in any given small portion of time, taking the former distance into the total weight, and again into the proper coefficient for adhesion on the rails, would give the resistance due to the foregoing. The second, which might be called the tangential resistance, was to be found in the excess of the average distance travelled over by the outer wheels, and the shorter distance gone over by the inner wheels. The formula was founded upon the consideration that the distance moved radially was to that moved over by the train, in any short space of time, in the ratio of the wheel base to the diameter of the curve. The tangential distance was in the ratio of half the gauge of the line to the radius of the curve, or, what was the same thing, in the ratio of the gauge to the diameter. The motion radially being thus as the wheel base, and tangentially as the gauge, these two being at right angles, the hypothenuse would be the actual distance rubbed over in the given time. The formula would then be as follows:—

Resistance of Curves.

Let a = gauge of railway.
„ b = wheel base.
„ c = ratio of adhesion to weight of train.
„ D = diameter of curve.
„ W = weight of train.
„ R = resistance of curve.

Then, 1st, where the axles were parallel and the wheels cylindrical,

$$R = \frac{W}{D} \sqrt{a^2 + b^2} \times c.$$

2nd, where the axles radiated, and the wheels were cylindrical,

$$R = W c \frac{a}{D}.$$

In both the above cases the effect of the centrifugal force was neglected, as the object of the formula was chiefly in its application to sharp curves when the motion was slow and the centripetal force of the engine's traction balanced very nearly the centrifugal force.

Mr. ROBERT TREFUSIS MALLET referred to a tramway constructed for Mr. Crampton on the Ottoman Railway on which he had been engaged, which he believed showed the extreme limit of what could be done on inclines by ordinary locomotives without special means of adhesion being applied, as in the case of the central rail on the Mont Cenis line. There was a gradient of 1 in 11 with curves of 500 feet radius. One difficulty was experienced in consequence of the extreme severity of the gradient; that was, the necessity of turning the engines at the summit, to avoid laying the fire-box bare by the gravitation of the water to the lower end. The engines could only pull one loaded wagon up this incline. The results were not brought forward as an instance of the greatest economy that might be obtained by using special engines, but rather to show what could be done with existing plant. He presented to the Institution diagrams showing the section of the tramway and the statistics of the engines used.

Mr. T. R. Crampton said there was nothing peculiar about the incline just referred to, but it had acted most perfectly. The gradient was 1 in 11 for a length of 2,400 feet, 600 feet of which had curves of 400 feet radius, descending about the same distance with inclines of 1 in 15 and 1 in 20, the total length being about one mile. About 100 tons per day were taken over this gradient of 1 in 11, and about 10,000 tons had been carried over the whole length up to the present time. The gross load was about 23 tons, of which the engine weighed 10 tons, leaving 13 tons of working load behind the engine. The adhesion was about one-fifth, so that the maximum was almost attained.

Mr. E. A. Drew remarked that the cost of working over the whole of these inclines—that was up half a mile of 1 in 11, and down half a mile of an average gradient of about 1 in 15, over which the loads and return empties were transported—was at the rate of two shillings per ton per mile. That tramway was successful, inasmuch as it served the end intended, by transporting, during the six months it was in operation, about 10,000 tons of material, which could not possibly have been carried by any other means so economically. Taking into consideration the price of coals there, and the difference in the wages of skilled labour, it was probable the same end under similar circumstances could have been achieved in England at half the cost. The engine was not made specially for this incline, but was an ordinary contractor's tank engine, built by Mr. Hughes, of Loughborough, the principal dimensions of which were—

Diameter of cylinders . . .	11	inches.
Length of stroke	18	„
Diameter of wheels	30	„
Wheel base	54	„
Four wheels coupled.		

The weight of the engine complete was 10 tons.

With a pressure of steam of 90 lbs. to 100 lbs. per square inch, the gross weight taken up the incline of 1 in 11 was 23 tons, including the engine.

Mr. Hawkshaw, Past President, thought Mr. Drew was under a mistake with reference to the cost of working these inclines. It showed how frequently mistakes were made in dealing with estimates. He could imagine a ton of goods costing two shillings for half a mile or a mile, if the engine used had to be kept in steam all day and only employed occasionally to take up a load of goods; but it could not cost two shillings to take an engine up 800 yards of incline of 1 in 11. In some cases, no doubt the effect of steep gradients might be to increase very greatly the cost of transit. If they occurred, for instance, on a coal line, where large loads could otherwise be taken, the intervention of these inclines would be objectionable. In other cases where frequent loads of moderate weight were dealt with, steep gradients did not increase the cost in the same ratio. Therefore, unless the nature and amount of the traffic, the frequency of the trains, and the necessity for frequent trains, were known, but little could be made of these questions of cost. Taking the railways of this country generally, they varied greatly in gradients. On one railway, the Lancashire and Yorkshire, for instance, which had steep gradients almost throughout, it would be found carrying at a certain amount of profit and working at a certain percentage of cost. On another line with good gradients, such as the North Eastern or the Great Western, it would be found that the percentage of expenses and the amount of profit did not greatly differ from that in the other case. That merely showed, having regard to average loads, gradients had not that effect generally which obtained in special cases where they had only great loads to carry. The question of steep gradients was much discussed twenty years ago, and it seemed to be raised now as if it were new. He could give a little information with regard to some steep gradients which had been worked for many years with the heaviest traffic perhaps in the kingdom. Some of the statements put forward as doubtful, or remaining to be proved, had been satisfactorily proved by long experience. Between Manchester and Oldham, for instance, the traffic was enormous. That railway consisted of gradients beginning with 1 in 59, 1 in 49, and varying from that to 1 in 124 till it approached Oldham, where the gradient rose to 1 in 30 and 1 in 27. Inclines of 1 in 30, or 1 in 40, at the time that railway was made, were considered unsuitable for locomotives, and those inclines were accordingly fitted with apparatus to get rid of the difficulty. He had occasion to remove that apparatus, and since that period locomotives alone were relied on, and from that day to this the traffic had been worked satisfactorily. The locomotives were nothing very unusual: they were of the sort commonly used on the Lancashire and Yorkshire line, and their performances were as follows:—The engines for ordinary trains had six wheels coupled, and weighed 29 tons each exclusive of the tender; they ordinarily took a load of 80 tons, exclusive of engine and tender, to Oldham; but in slippery weather 15 or 20 tons less. Another class of engine used for heavier trains had also six wheels coupled, weighed 33 tons 13 cwt. exclusive of tender, while both together weighed 49 tons. These took up a load of about 120 tons exclusive of their own weight; and in slippery weather about 20 tons less. These classes of engines had been working for many years; and for such traffic it was clearly not worth while for the company to lay out a large sum of money to get rid of those gradients. The question as to how far extreme gradients might be pushed, was one that could not be solved unless all the details were known of the nature and extent of the traffic; but it was quite clear that for a traffic like that between Manchester and Oldham the gradients he had mentioned were worked without difficulty. On some other parts of the line there were gradients of 1 in 44, 1 in 50, &c., and several of the important towns on the line were approached by such inclinations, while the traffic was second to none in the kingdom in magnitude; and yet the average working expenses were not greater than on other lines which had not these heavy gradients. But, in reference to the question of expense, he did not wish to be understood to say, that gradients of 1 in 27, or 1 in 30, were good, and to be adopted lightly, or that there might not be cases in which it would be better to lay out a large sum to get easier gradients.

With regard to engines, he might mention that some were now being constructed under his direction for the Mauritius Railways, which had eight wheels coupled, and weighed 47 tons exclusive of tender, by which he got the whole adhesion due to that weight. The total wheel base of these engines was only 15 feet 6 inches. He expected they would work well on gradients of 1 in 27. The radii of some of the curves were 1,000 feet.

Mr. Crampton, in explanation, stated that the total distance run at the expense of two shillings per ton was two miles—viz., from the commencement of the gradient of 1 in 11 to the end of that of 1 in 20, including the return empty. He agreed that it was useless to give any particular expense of working, unless all the circumstances of the traffic were at the same time taken into consideration. It might be assumed that this incline could be worked for an expense of something like 40*s.* per day for haulage at the outside, and carry 100 tons. He was not aware of 1 in 11 having been worked so successfully on any previous occasion with an ordinary engine. The climate was good. The cost of working such an incline could be calculated to a certainty according to the circumstances of the case. He had decided to use this incline, as no other means presented itself which would enable the material to be carried over the mountain to complete the railway in the time required.

Mr. G. R. Stephenson said, about twenty-five years ago, when the Oldham branch of the Lancashire and Yorkshire Railway was being made, he had the management of the designs for that work under the late Mr. George Stephenson and Mr. Gooch. As Mr. Hawkshaw had stated that he had to alter certain works that were executed, it was only just to the memory of those who originally carried them out that he should explain the cause of the alteration. The Oldham branch was made at a comparatively early period of the locomotive engine, which was not then so well understood as now, and when it was attempted to serve by branches the traffic of towns lying at some miles distance from the main line. It was at that time considered that the Oldham branch would form the terminus of the line in that direction, and therefore it was thought expedient to use the most economical plan of working the incline, which amounted to 1 in 27, and that of the rope was considered to be so, as well as the most simple. The plan of working was this:—on the arrival of the train at this part of the incline, it was pulled up by a rope round a large pulley at the top of the incline, balanced at the top by the weight of trucks and engine attached, which assisted the ascending train; and the consequence was the incline was worked with great economy, great regularity, and with perfect success. But the time came when the public traffic demanded that the line should be carried beyond Oldham, and, therefore, it was evident that the rope system combined with the locomotive would be expensive as well as fraught with peril. When the line was extended, it was found as a matter of course that it could not be worked successfully with a mixture of the rope and locomotive systems, and the former was consequently abandoned at a period when fifteen or twenty years' experience in the improvement of the locomotive had made it capable of doing that which it was formerly unable to accomplish.

Mr. John Clark thought that the retarding force to the passage of railway vehicles round sharp curves arose from two causes;—first, from the unequal lengths of the outer and inner rails; and secondly, from the change in the position of the train. The action of these causes of retardation had been illustrated, during the discussion, by comparison with the action of the screw, but he thought the comparison did not hold. He considered the retarding effect was equal to the friction due to the weight of the vehicles when moved, as on a metal base plate, to their altered position. He had introduced a system of construction applicable to railway vehicles, which permitted all the axles of each vehicle approximately to radiate to the centre of curvature of the rails. This mode of construction had been applied by Mr. Edward Woods upon a South American railway to a considerable number of wagons and carriages, and had worked satisfactorily for the last eighteen months. These vehicles had six wheels; the two endmost axles were free to turn in the horizontal plane about the centres of their length, so as to permit their movement into positions normal to the curve of the rails. This was effected by the pressure of the inner rail against the flange of the inner wheel upon the central axle, which was free to move endways parallel to itself. The amount of this endlong movement of the central axle was always equal to the versed sine of the circular arc whose chord was equal to the wheel base of the vehicle. A system of linkwork connected the axle-boxes of the central axle with those of the two outer axles, and it was so adjusted that the endlong movement of the central axle imparted, through the linkwork, a motion to the axle-boxes of the outer axles, which placed their centre lines approximately normal to the curve. The vehicles were adjusted to pass round curves as sharp as 60 feet radius. The length of the wheel base in these wagons was 12 feet, but there was no reason why a much longer wheel base should not be adopted. Some vehicles designed by Sir Charles Fox and Son for the Queensland Railways, upon this system, had a wheel base of 30 feet, the length of the vehicle being 48 feet, the gauge of the railway 3 feet 6 inches, and the sharpest curve 5 chains radius. In a mile of continuous curve of 5 chains radius, on the ordinary gauge, the length of the outer rail exceeded that of the inner rail by about 80 feet, and he considered that, if the axles radiated to the centre of the curve, the usual 'coning' of the wheel tires made up for about 60 feet of this slip; on a curve of 7½ chains radius they were nearly equal. But unless the axles radiated, he thought that the 'coning' of the wheel tires provided no compensation for the excess of length of the outer rail.

Sir Charles Fox, in referring to the observations made by Mr. Hawkshaw, said a few months ago he went to Oldham to inspect the incline plane of 1 in 27, on the Lancashire and Yorkshire Railway Company's branch to that town, and was not a little surprised at the admirable manner in which the ascent of that incline was performed by the locomotive. There was one point which caused him some amusement, if not instruction. On that occasion he asked the enginemen whether, in going up the incline, they ever had such a train load as to be beyond the power of the steam in the engine. They replied that such was the case occasionally, and in that event they put on the breaks, and waited till the steam got up sufficiently to enable them to walk up to the top of the incline. They added that they never had to run back with their trains. The boilers of their engines being strong and good, they had great confidence in them, and so they waited on the incline till there was sufficient pressure of steam attained to take them up.

The other subject he would refer to was to the opinion expressed by Mr. Hemans, as to the inapplicability of the atmospheric system to steep inclines; but he did not understand that gentleman to apply the same remark to the pneumatic system. He looked upon the atmospheric system as having failed from several causes, two of the most serious being the following:—

First of all, there was extreme mechanical difficulty in keeping the long valve in good order, so as to be air-tight; and secondly, the evolution of heat that took place in the air-pumps, from the air exhausted from the tube being rarefied, and having its capacity for caloric thereby increased, made them so hot, as to prove a serious obstruction to their proper lubrication. If ever there was a good opportunity of making experiments on the amount of latent heat in air of different densities, that was certainly one of them. Sir Charles Fox looked upon the pneumatic system as perhaps the best that could in many cases be adopted for steep inclines; as in that case no locomotive would be employed, and the power required for lifting it from the bottom to the top of the incline would be saved; and as, by having recourse to this means of traction or repulsion, carriages of a much lighter character than those at present in use on railways might be adopted, obtaining thereby a far more favourable proportion between the dead and living load than now existed. As no power would be expended upon the air in the tube except at starting, and in overcoming its small friction against the sides of the tube when in motion, he considered that this system would be worked with economy, and certainly with safety. In working a railway by the atmospheric system, it was often found necessary to use a vacuum which was represented by a column of 15 feet of water, or about 15 inches of mercury; but on the pneumatic system, an experiment made on the line at the Crystal Palace showed that a dead weight of 30 tons could be taken from a state of rest, up an incline of 1 in 15, at the rate of 12 miles an hour, with a vacuum which did not represent more than a column of water of 6 inches or 8 inches, which was only one-thirtieth, or one twenty-second part of what was found necessary in working the atmospheric system. Looking at it as a whole, he believed the pneumatic system was the one best adapted for surmounting the heavy gradients which were necessary in crossing Alpine passes, &c.

Mr. G. W. Hemans quite concurred in the opinion that the pneumatic principle was most worthy of consideration for the working of these inclined planes, and that the atmospheric system was utterly unsuitable.

Mr. J. W. Grover remarked that, though his practical experience in working gradients did not extend beyond those of 1 in 45, yet he had reflected upon the subject a good deal. If it was wanted to get as much friction as possible out of a certain weight, whatever it might be, and the weight were resolved into two forces, each acting at an angle of 60° with the vertical, the pressure on any substance at right angles to them by each of these forces would be equal to the weight; and with both, of course, they would produce the effect of doubling the weight. This result would be effected by

V grooves in the driving-wheel. The same point had occurred to other Engineers fifteen or twenty years ago, but no one had ever tried the thing in practice, and he brought it before the Institution now, in the hope that some gentleman possessed of an engine might be induced to make the trial, by turning up the tires in the way he had pointed out. The grooves were cut V-shaped; and if a roundheaded rail were used, the principle of the wedge would come into operation by the two bearing surfaces on the rail; and if the angle of the groove were 60°, double the effect of the weight of the locomotive would be obtained without actually increasing the weight of it at all. Mr. Grover illustrated his views by reference to a model, in which three equally weighted discs of wood represented wheels with the ordinary concave form, and V grooves cut at different angles; a moveable strip of wood, with a rounded upper edge, represented the rail, and was raised or lowered to show various gradients; the wheels being placed upon the rail, and prevented from turning on their axles by means of keys, the rail was gradually raised at one end, until it assumed such a slope as to make the disc slide down it, thereby representing relatively the gradient that the respective wheels would climb. By this it was shown that, whereas the ordinary form of wheel in wood would slide on a gradient of 1 in 7½, the V groove of 90° would just move on 1 in 6, and the V groove of 60° on 1 in 4. Several patents had been taken out involving this principle, but nothing practical appeared to have been done.

Captain Tyler, in reply upon the discussion, said, in the first place, he had referred at the commencement of his Paper to the system brought forward by Mr. Grover. He had said:—"Grooved wheels afford obviously increased bite; but there must be, when they are used for locomotive purposes, continual abrasion from unequal travel of the surfaces in contact, with increased friction on curves, and some loss of power, in proportion to the increased bite obtained, from what may be termed back-adhesion." He thought he had described fully, though briefly, in that sentence, the real difficulties in the way of the application of that system.

The most important question of all was, perhaps, that of the coefficient of adhesion. He understood Mr. Conybeare to say that it was independent of climate; but he thought the widely varying opinions which had been expressed showed very clearly that the contrary was the case,—viz., that the value of that coefficient varied, as might be expected, to a great extent in different climates; and he thought that such variations from climate afforded the only explanation by which to account for the wide differences of opinion that had been expressed. Mr. Cowper had produced an ingeniously simple instrument for testing the adhesion to some extent of iron upon iron; but he had adopted, in the experiment before the Institution, the process of filing the rails. He had taken the opportunity of making some experiments privately with this apparatus; and he found he could not get the "rolling load" to stand at all on an inclination of 1 in 4 till he took a file in his hand and used it pretty freely. He had cleaned the grease off, but until he used the file he could not make it stand at 1 to 4; and he observed that Mr. Cowper had also used the file before he could make his moving load stand on that gradient. It was quite clear that it could not be expected, in practice, to file wheels and rails before going up an incline, and to keep them filed for the purposes of railway traffic.

On the Mont Cenis there were greater difficulties, in one respect, than were met with under ordinary circumstances in other places. It would hardly be supposed that the item of dust would, at or near the summit of Alpine ranges, materially affect the question of adhesion; but the fact was, that the fine dust which was blown from the road upon the line in dry summer weather was of a most slippery description. It was the dust of the schistose rock with which the roads were mended, and when it was wetted it caused the rails to be in a worse condition sometimes in summer than they were from the snow on a winter's day. When the snow was swept off the rails in winter, it left them clean and dry, so as to yield an adhesion of one-sixth; whereas this dust, combined with moisture, reduced the adhesion to one-eighth. But, whatever the exact coefficient under different circumstances, it was clearly important, on this extreme gradient, to allow a greater margin for adhesion than on more moderate gradients.

He would, in the next place, advert to the Navigation incline, to which he had alluded in his remarks following the reading of the Paper. Mr. Naylor had brought forward some figures stated to have been received from the Locomotive Superintendent of the Taff Vale Railway Company, which did not quite agree with those he had produced from Mr. Fisher, the Engineer of that Company. He had read, from Mr. Fisher's note, that the two engines, with six wheels coupled, which worked that incline, weighed when loaded 36 tons; and that the maximum load of each, on an average gradient of 1 in 20 for half a mile, was 40 tons, and the regulated load 25 tons; and he had stated that the coefficient of adhesion which had been calculated upon in the latter case was about one-tenth. Mr. Naylor said he had received a higher regulated load, and different figures in other particulars. Captain Tyler had written again to Mr. Fisher, who had also communicated with the Locomotive Superintendent, Mr. Tomlinson. From this correspondence it appeared, that the regulated load of 40 tons, referred to by Mr. Naylor, had, after a mishap, been reduced by Mr. Fisher to 25 tons; though it would also appear, from Mr. Tomlinson's statement, that the men had not of late worked in practice to a load so low as their instructions required.

So much, then, with regard to gradients which had been worked by a locomotive for passenger traffic in this country. The gradients which had mostly been referred to varied from 1 in 27 to 1 in 40, and 1 in 50; but these gradients could hardly be compared with that mentioned by Mr. Crampton, of 1 in 11, with those which had been temporarily used in America, or with those on the Mont Cenis Railway.

In speaking of certain railways in South America, Mr. Lloyd had expressed the opinion, that it would rarely be necessary to employ a steeper gradient than 1 in 20 in crossing mountain ranges. He supposed Mr. Lloyd applied that remark to the Alps, as well as to other mountains. But it was not, in practice, so much a question of what could be done if money were no great object. The question was, rather, whether, having a certain height to surmount, it was better worth while to go to the greater expense of making gradients of even 1 in 20 for longer distances, or whether it was not more advantageous to make a shorter line with steeper gradients, at a greatly reduced cost. Whatever might be the ruling gradient—1 in 20, or 1 in 12—the same height would in such a case have to be overcome, and the same depth to be descended again; and when, by constructing a shorter line at a cheaper rate, with gradients of 1 in 12, the journey could at less speed be made in as short a time, he did not see why it should not be done. He would demonstrate his meaning by the aid of some figures, showing the relative cost of certain lines with different gradients and curves on some others of the Alpine passes.

For the passage of the Simplon, with gradients of 1 in 25, and curves of 200 mètres radius, the estimated cost for 255 kilomètres, from Bouveret to Arona, was 156,410,000 francs; while for the same pass, with gradients of 1 in 16, and curves of 150 mètres radius, the estimated cost was only 130,410,000 francs; and with gradients of 1 in 12, and curves of 40 mètres radius, the cost of a line, laid on a portion of the public road where available, was estimated at only 65,000,000 francs.

For the Lukmanier Pass, from Coire to Bellinzona, with gradients of 1 in 40, and curves of 300 mètres radius, the tunnel line, 116½ kilomètres long, was estimated to cost 223,282,108 francs; while in the case of a summit line for the same pass, without any public road on which to lay it, and therefore with all new works,—with gradients of 1 in 20, and curves of 150 mètres radius,—the cost for a length of 145 kilomètres was estimated by the same Engineer at 53,749,954 francs.

Much of the discussion had related to the effect of curves, and to the reasons why engines and trains did not travel round them so easily when the curves were sharp; but those remarks had not been accompanied by any good practical suggestions for getting over the difficulty. Mr. Fell had done so to some extent on the Mont Cenis experimental line, by flattening the gradient on the curves; and the question was asked, to what extent this had been carried out. Mr. Fell had so far altered his original section that, on very sharp curves, he relaxed the gradient from, say, 1 in 13, to 1 in 15, or 15½, at the same time that he increased the gradient on the straighter portions to about 1 in 12: he then found that his engine gained speed on the curves, and that, in fact, he had done rather too much. But the circumstances of this case differed materially from those which were ordinarily met with. The various vehicles—of short wheel-base—travelled with extra ease round the curves of this line, because the horizontal guide-wheels of the wagons and the horizontal wheels of the engine, acting upon the central rail, tended to keep them in their proper positions on the curves, and to prevent some of the prejudicial action from coming into play. When very sharp curves were used—and he believed they must be used more and more—it was thought necessary, under the prevailing system, to employ engines of short wheel-base, and that necessitated overhanging weights in front of the leading and behind the trailing-wheels, and led to the use of an objectionable kind of engine. And there were other objectionable things, to which he would take the opportunity of referring. The weight on the driving-wheels was frequently so enormous, that it became a matter of difficulty alike with the Engineers who had to construct, and with those who had to maintain a railway. He thought it was time some decision was come to as to the limit of weight that should be placed upon each pair of wheels. He was afraid, now that steel tires were so extensively used, those weights would be greater than ever. Locomotive constructors had hitherto been checked by the tendency of wrought iron to be crushed. There was a risk of squeezing the tires out when they were loaded beyond a certain point; but now that steel tires had come into common use, he feared the weights on the driving-wheels would be further increased, and that still heavier tank-engines, with overhanging ends, would inflict greater strains on the bridges and the permanent way.

He thought Mr. Conybeare had been rather hard upon Mr. Fairlie's engine, the principles of which tended to remedy the evils he spoke of. The system of employing a double engine, placed upon two bogie frames, could not be regarded as novel, for it was applied to every railway car in America, and by that means they went round curves, and over inferior descriptions of permanent way, better than they would otherwise do and with far greater safety. Mr. Fairlie further connected two boilers together upon one frame. The difference between this application and that employed on the Giovi incline was very great. Mr. Fairlie fastened the boilers together at the ends and worked them as a rigid mass upon moveable bogies, while on the Giovi the two tank engines were run fire-box to fire-box with separate boilers and framing. He would not assert that Mr. Fairlie's system was the best that could be devised, but it possessed an advantage on curves in drawing from the bogie frame instead of from the framing of the engine, and it was well worthy of consideration and further experiment. M. Thouvenot was endeavouring to get a similar system adopted on the Continent; and had prepared for the Paris Exhibition the diagram of an engine on this principle, added to the system of Mr. Fell. M. Lömmel, the Engineer of the Lukmanier Railway, proposed to adopt such a combination on that line; and it appeared, therefore, that this form of engine was likely to have a fair trial on the Continent, as he hoped it might also have in this country. The defect of the leakage of the steam pipe would no doubt be overcome by special arrangements, and the difficulty of equalizing the draught up the two funnels, either by the mid-feathers which were to be placed between the fire-boxes, or by separating the fire-boxes completely from one another.

He would now say a few words with regard to the atmospheric and pneumatic systems as applicable to the working of steep gradients. He had not intended to refer in the Paper to the old atmospheric principle, which he had looked upon as a thing of the past; and he was surprised to find that Mr. Pole and Mr. Margary still thought it should not be abandoned. Sir Charles Fox was evidently of opinion, as he believed others were also, that the pneumatic system was applicable to very steep gradients; and he had said that in applying it to steep gradients, there was nothing to overcome except the dead weight of the train itself. He did not suppose, however, that Sir Charles Fox meant that the air did not weigh something. With the central rail an engine of 16 or 20 tons had to be taken up the incline and to be got down again. Some said that was a wrong principle; but whatever was used there must be weight of some kind, whether in a rope of air or a rope of wire; and in point of fact the weight of air was very considerable. For instance, the weight of air in a tube one mile long with a sectional area of 60 feet—which was a small one for the pneumatic system—would be ten tons; and the weight of air in a tube three miles long would be 30[1] tons,—a greater weight to set in motion than the locomotive itself. Allowance must also be made for the pressure of the air in the tube, which would be higher than that of the atmosphere.

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[1] These being the weights of the air *in vacuo*, the actual weight of air to be drawn up an incline would be computed in proportion to the excess of pressure inside the tube over that of the atmosphere.—H. W. T.

Several questions had been asked with regard to the snow. This would have to be provided against, on the Mont Cenis Railway, in the case of avalanches, drift snow, and fallen snow. With regard to avalanches it was pretty well known at what runs they generally came down, and at those places, as Mr. Brunlees had explained, masonry covered ways were to be provided, backed with rubble, so as to allow the snow and other matters to slide over them. In the case of drift snow, which sometimes attained in known positions to a depth of—say 70 feet, strong wooden covered ways must be provided. Then there was the case of ordinary snow fall. Where there was no drifting, the fallen snow was not very deep. Mr. Abernethy had said that such a line should be carried over the summit completely under cover. But he thought that was hardly necessary, for he understood that the snow never laid to a greater depth than 3 feet over the greater portion of the summit of the pass. There were, in some respects, both advantage and economy in clearing away the snow on parts of this line, which ran more or less along the sides of precipices. Instead of having to remove the drift snow as was sometimes required in this country from a deep cutting, involving a difficult and laborious operation, it had only to be pitched on one side or the other down the precipice. There would be, he believed, less difficulty in clearing the snow from the line than was at first expected ; and the engine in going down these steep inclines, would have its own weight to assist it in pushing the snow away.

The difficulty of level crossings was not easy to get over. Mr. Brunlees had explained the mode he proposed of making the centre rail moveable. But it was a question whether that could not be done better with a vertical action than a parallel ruler action. He thought the rail might perhaps be so connected with the gate, that it might sink down when the gate was open, and be raised up when it was closed, by a self-acting arrangement.

It was proposed to place the chairs, which rested on the longitudinal sleepers and carried the middle rail, not over the cross sleepers, but between the cross sleepers, and to fasten those chairs down to the longitudinal sleepers with wood screws. There were also longitudinal ties in front of these chairs fastened with through-bolts to the longitudinal timbers. He would wish to see through-bolts employed instead of merely wood screws to secure the chairs of the central rails to the longitudinal timbers, in order to obtain sufficient power to resist lateral action upon the central rail. He thought wood screws through the longitudinals only would not effect that object, while through-bolts through the chairs, the cross sleepers, and the longitudinals would be efficient. He was the more anxious to make this observation, because it would be found he stated in his official report upon this railway two years ago—"The safest portions of the proposed railway ought, indeed, under proper management, to be those on which the gradients being steeper than 1 in 25, the middle rail will be employed. There is no difficulty in so applying and securing that middle rail, and making it virtually one continuous bar, as to preclude the possibility of accident from its weakness or from the failure of its fastenings, and the only question in my mind is whether it would not be desirable still further to extend its application to gradients less steep than 1 in 25. It would apparently be advantageous to do so, not only for the sake of obtaining increased adhesion with less proportional weight, but also with a view to greater security, especially on curved portions of the line." He considered the safety of the system depended upon the middle rail, which should be thoroughly well secured in its place and protected against the effects of lateral action. The bolt through the tie and the middle of the longitudinal timber would not give lateral steadiness; he wanted to see a bolt from each side of each chair through the transverse as well as the longitudinal sleeper, to give lateral steadiness. From the experience which had been obtained with wood screws he had not much faith in them. They were used on the Great Western Railway in the first instance, but Mr. Brunel had them all taken up, and there was not now a wood screw on the line, through-bolts being used from end to end of the broad-gauge system.

Mr. Brunlees remarked, that Mr. Abernethy had stated that in mountain passes, where so much covered way had to be introduced, it might be as well to resort to the ordinary mode of tunnelling at once. Mr. Brunlees would state in explanation that in the case of the Mont Cenis, the cost of the various plans for crossing would be as follows, viz.: for the Grand Tunnel of the Alps, now making, the estimate was over $2\frac{1}{2}$ millions sterling. The cost of ten miles of covered ways in masonry for Mr. Fell's system would be £176,000; and the covered ways, as actually being carried out in timber and iron, would be under £50,000.

Mr. Brunlees would also remark, in reference to the floods alluded to by Mr. Abernethy, that the French government were now repairing the whole of the road in a most substantial manner at their own cost, and in addition to that would pay to the Mont Cenis Railway Company two-thirds of the loss sustained by them, and this did not exceed £8,000.

Mr. Fell's locomotive possessed the great advantage of getting rid of dead weight. This was always a matter of consequence, but in such a case as the Mont Cenis, where a high summit had to be overcome, he considered it of vital importance. Mr. Fell's engine of 16 tons weight, with the 24 tons pressure obtained from the horizontal wheels, gave an adhesion of one and a half times more than that derived from the weight of the engine itself; so that with a coefficient of a fifth the total adhesion obtained was equal to one-half the weight of the engine. He felt much indebted to Mr. Fairlie and others who had taken up the subject of distributing the weight on the driving-wheels of locomotives, as he considered that the English system of permanent way was quite unfit to carry 18 tons on a single pair of wheels, which he understood was the weight now used on some railways.

Mr. George Edwards communicated, through the Secretary, the following remarks:—He had been resident among the Alps about fourteen years, and was first occupied in the construction of the Victor Emmanuel Railway, in Savoy, which wound its way among the lower spurs of the Alps until stopped, by the steepness of the valley, at about 2,300 feet above the level of the sea. He was led to consider how this line could be connected with those on the other side of the Alps; and he had aided the late Mr. Thomas Bartlett in completing and trying his drilling machine, which, slightly modified, was now used in boring the Great Tunnel under the Mont Cenis. His attention, however, had been principally directed to the application of the water power, which abounded in the Alpine districts, and which was proportioned to the steepness of the valley. Where a railway following the valley would require a gradient of 1 in 10, the water power available, with an equal amount of diversion or damming, was ten times as great as when the valley only fell 1 in 100, and this exactly corresponded with the difference of tractive force required. How to apply this power advantageously to the traction of trains was, in his opinion, a problem worthy the serious consideration of Engineers. Since long tunnels like that under the Mont Cenis seemed to be out of the question, from their enormous expense, and the long time required for their construction, it was evident the line would have to be carried higher up the mountain to shorten the summit tunnel or surmount the pass. Personal observation in all seasons of the year had led him to conclude that, whatever system of railway was made at an elevation exceeding an average of about 4,000 feet above the level of the sea, it must be protected by a covering of some kind, if regular service during the winter months was to be depended upon. Every portion of the line would be subject to heavy snowfalls, and a great part of it to snow-drifts, which in a few seconds were sufficient to bury a train and defy all snow-ploughs and manual labour that could be brought to bear upon them, and another part was subject to avalanches and slips of earth and rock. Where the line from its position was not subject to avalanches or rock slips, a light covering only would be necessary; but to guard against these avalanches and slips, he would strongly recommend, wherever practicable, tunnelling from horizontal side headings into the mountain at a few yards from its surface; and, where open-cut tunnels were made of masonry, to cover them with several yards depth of earth. Very frequently in spring large portions of rock were detached from the mountains, and, falling from a great height, swept all before them. He had witnessed this on his own works, where, on one such occasion, a block of about 300 tons of rock fell from a height of 1,000 feet, breaking into pieces as it bounded from ledge to ledge, but leaving blocks of sufficient size to break the rails of the permanent way like so many bars of glass.

Snow-drifts, though less dangerous, were of more frequent occurrence than rock slips, and were very annoying. He had been dragged over some of them in the courier sledge on the sides of a slope of 1½ to 1, while twenty men, unable to cut a benching large enough for the sledge, held it from turning over by ropes attached to the top. Taking an average from some of the principal Passes of the Alps, as the Mont Cenis, Simplon, St. Gothard, Lukmanier, St. Bernardino, Splugen, and Septimer (all of which he had passed, and most of them during winter), it would be found that ordinary railways might generally be made to an elevation of about 4,000 feet above the sea, without the necessity of lengthening the line for the purpose of reducing the gradients. At this elevation, however, the circumstances changed rapidly; the valleys rose faster, or left the natural course of the line altogether. Snow, avalanches, and slips were of frequent occurrence, and pointed to the expediency of shortening the line as much as possible. Thus the question of steep gradients was introduced. Reverting to the above-named Passes, it would be found that above the elevation of 4,000 feet, the average distance over the mountain by following the natural sinuosities of the ground and a mean gradient, was from 15 to 20 miles.

The mean elevation (over the 4,000 feet) was about 3,300 feet.
The mean gradient . . . . „ 1 in 12 to 1 in 16.

With an average gradient of 1 in 12, or a maximum gradient of 1 in 10, and from 15 to 20 miles of covered ways, the difficult part of these Alpine Passes might be crossed by railways passing over their summits. After studying various systems on steep inclines, such as the locomotive system, the rope system, and the old atmospheric tube system, he was driven to the conclusion that none of them avoided the necessity of the covered ways mentioned. He therefore thought that the wisest plan to take, was to make use of them for transmitting the water power by means of compressed air on the pneumatic system (as it was now called), and propel the train from the bottom to the top of this difficult part of the mountain.

Having mentioned this idea to the Minister of Public Works of

Italy, he had been invited to submit to a committee a memoir[1] developing this subject; a copy of which unpublished pamphlet, in French, he had now the pleasure to present to the library of this Institution.

March 12, 19, & 26, 1867.

JOHN FOWLER, President,
in the Chair.

The discussion upon the Paper No. 1,160, "On the Working of Steep Gradients and Sharp Curves on Railways," was continued throughout these meetings, to the exclusion of any other subject.

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[1] "Systême de Chemin de Fer Hydro-Pneumatique pour le Passage des Hautes Montagnes." 4to. Turin, Octobre, 1865.

April 2, 1867.

JOHN FOWLER, President,
in the Chair.

The following Candidates were balloted for and duly elected:—Charles Napier Bell, John Frederick Bourne, John Edward Boyd, William Dennis, John Marley, William Martley, and Thomas Robert Shervinton, as Members; Thomas Charles Clarke, William Donaldson, M.A., William Hartree, Henry George Hulbert, Thomas Jackson, Jun., Edward Davis Mathews, Henry Beadon Rotton, Peter Thomson, Thomas Andrew Walker, and John William Watson, as Associates.

No. 1,081.—"Memoir on the River Tyne."[1] By William Alexander Brooks, M. Inst. C.E.

Before considering the Tyne as a tidal river, a short account of it, as the outlet for the drainage of about 1,142 square miles of country, will enable a judgment to be formed of the extent of the influence of the land-floods to which its channel is subjected.

The water-shed of the Tyne has a mean elevation of about 500 feet above the sea, and the higher sources about 1,200 feet.

The North and South Tyne have an average fall per mile of about 15 feet until they unite at Hexham, from whence to Stocksfield, a distance of 9 miles, the fall is at the rate of 7 feet; and to Ryton, 7½ miles lower, at the head of the tidal flow, the fall is $3\frac{8}{10}$ feet per mile. From Ryton Weir to the bar of the River Tyne, distant 19½ miles, the fall is 16 feet 9 inches to low water of spring-tides on the bar, or an average of 10⅓ inches per mile.

The lower tidal portion of the river, or from Newcastle Bridge to the bar, a distance of 10½ miles, has a fall between the relative low waters of each station of 3 feet 6 inches, or 4 inches per mile; but if the sea-reach between the Narrows and the bar be excluded, then the fall to the Narrows, or the entrance of Shields Harbour, is only at the rate of 3 inches per mile. Between the Narrows and the bar, or in the short length of 1 mile, the fall is generally 12 inches during spring-tides, and at low ebbs at sea, frequently 18 inches.

During neap-tides, which, in the absence of freshes, often ebb out to nearly the same level as spring-tides since the shoals or sand-banks have been lowered, the fall is still less, because those tides at sea do not subside so low as springs, by from 18 inches to 24 inches. Hence, during neap-tides, there is much less opposition to the early flood current, and therefore the bar is not, at such times, raised in height, as might otherwise be expected, on account of the diminished quantity of tidal water passing over it, as compared with spring-tides.

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[1] The discussion upon this Paper extended over portions of two evenings, but an abstract of the whole is given consecutively.

If the depth on the bar of a river depended simply on the quantity of tidal water passing over it, it might be expected that the bar would rise in height during neap-tides; but this is not the case in rivers constituted like the Tyne.

These observations are only made to afford an opportunity of contrasting the Tyne with other rivers. The difference of level thus recorded merely represents the state of the river at the relative periods of low water at each place, and not the slope which actually exists at the same instant, or when it is low water, or first of flood at the bar; by which comparison alone a true test is afforded of the condition of a river, of its capability as a channel for drainage, as well as for the free reception of the tidal wave.

The effect of the inefficient drainage, in checking the ingress of the early flood-tide, is well demonstrated by the tidal observations of the 8th of September, 1854 (Plate 10^{D}).

On that day the morning's tide at Newcastle ebbed out to 2 feet 6 inches on the gauge, or 15 inches lower than the ancient low-water level of spring-tides. This low water was, therefore, 12 feet 6 inches below datum of high water of spring-tides. At Shields it ebbed out to 9 inches below zero, or to 15 feet 9 inches below datum; and on the bar, to 1 foot 10 inches below zero, or to 16 feet 10 inches below datum—that datum being the level of high water of the spring-tide of the 31st of May, 1813, which is marked in the masonry of the south-west angle of the Low Lighthouse, North Shields, and to which all subsequent levels and soundings have since that period been regulated.

The afternoon flood-tide of the 8th of September, 1854, rose to 14 feet 4 inches on the gauge at Newcastle, and to 14 feet 8 inches at sea; showing that the high-water level at Newcastle was on that day 4 inches below high water of that tide at sea.

It was low water, or first of flood at sea, at 10·20 A.M.; the water being, as above stated, 16 feet 10 inches below datum; but at this period of first of flood at sea, the tide was still ebbing at Shields, and was at the level of 15 feet 2 inches below datum, showing a fall of 20 inches between the tidal stations at North Shields and the bar, the distance between those stations being 1½ mile, or a fall of nearly 14 inches per mile. Again, at the time of low water at sea, or at 10·20 A.M., when the level was

16 feet 10 inches below datum, it stood at 9 feet 5 inches below datum at Newcastle, showing a fall of 7 feet 5 inches, or an average slope of nearly 8½ inches per mile, although at the time of low water at Newcastle, or at 1·25 P.M., the tide had fallen to 12 feet 6 inches below datum. At this latter period it was half-flood at sea.

At high water at sea, at 4·30 P.M., when at 14 feet 8 inches on the gauge, the tide had only risen to 13 feet at Newcastle, showing that 1 foot 8 inches of the depth of the tidal receptacle was unfilled; and if to that amount be added the difference between the level of the tide at Newcastle at the instant of low water on the bar, and that to which it ultimately subsided, as before recorded, the total loss of range at Newcastle will be found to have been 4 feet 9 inches; and that, of the apparent range of tide of 12 feet 2 inches, a depth of only 7 feet 5 inches was due to the column of tidal water thrown in.

On that day it was high water at Shields at 4·30 P.M., and at Newcastle at 5·25 P.M.; or the tide-wave traversed 10½ miles of a very circuitous channel, at the rate of about 11½ miles per hour. Practically speaking, as far as the purpose of navigation is concerned, no advantage is gained by a greater rapidity of the tidal-wave's progress.

The level of the tide at that day happened, as in the datum tide of 1813, to be the same both at Shields and at Newcastle.

When the Author first entered upon his duties as Engineer to the Conservators of the Tyne, in the summer of 1842, no record existed in his office of the tidal observations made for Mr. Rennie in 1813. Subsequently, when the late Mr. J. M. Rendel gave evidence before a Royal Commission, which had been appointed to inquire into the state of the Tyne, that Engineer showed, by his own tidal observations, that there existed a depression at Newcastle, amounting to from 2 to 3 inches below the level of the same tide at sea; and that, inasmuch as the tidal observations of Mr. Rennie, on the 31st of May, 1813, showed high water at Newcastle at the same level as at sea, the inference was, that the works of the Author had had the effect of checking the progress of the tidal wave. It was, however, fortunate for the Author that he, in the early period of his duties, had made tidal observations which proved that a small but constant depression existed at Newcastle, when the river was clear of the presence of any excess of drainage water; but still more fortunate that, during the year preceding the inquiry, he had been enabled to obtain the original tidal sections as tabulated in 1813 (Plate 10^{B}). That document was produced before the Royal Commission, and a reference to a few of the tidal observations proved that, in 1813, it was not unusual to find depressions existing at Newcastle, even to the extent of from 7 inches to 10 inches below the level of high water at sea. It is hardly necessary to observe that the late Mr. Rendel, with his usual candour, at once admitted the evidence as conclusive in favour of the river works.

Mr. Ure, who succeeded the Author as Engineer to the Tyne Commissioners at Christmas, 1858, selected a few days for his observations in 1860, when the river was clear of 'fresh,' and the result then arrived at was that, on an average, the high-water level at Newcastle attained the same level as at sea, and that the low-water surface at Newcastle had been depressed to the extent of from 14 to 18 inches.

The present range of tide at Newcastle may be, therefore, estimated as about 2 feet greater than in 1813.

As the acceleration of the rate of progress of the tidal wave is a necessary consequence of the great increase of the hydraulic mean depth of the channel, consequent upon the Author's works, it will not be necessary to dilate on this feature; and the more so, because of the meagre nature of the ancient tidal observations, the existing records having relation solely to the transmission of the high-water level.

The great variations in the tidal phenomena, which take place at different stations in the same river, are a fertile source of error as to the progress of the tidal wave. Thus, at sea, the interval of rest is short both at low and at high water, varying from three to six minutes; whereas, in the channel of the river, local features produce considerable influence in prolonging the duration of the current, as well as that of the period when the tide remains at the same level. Careful observers will, on the approach of the times of low and high water, make still closer records of the level of the tide, noticing at the same time all changes that take place in the direction of the current, as well as the exact time during which slack or still water remains, as in the tidal example before alluded to, of the 14th of September, 1848 (Plate 10^c).

Further, on the subject of tidal observations: it is usual to estimate the progress of the tidal wave solely by reference to its transit at the time of high water, and to look upon it as a true test of the condition of a tidal river, or as to whether it is in a good or bad state. This is, however, an error, because the rate of progress of the tidal wave varies throughout the whole period of the tide, just as it happens to be affected by the changing features of the bed of the river. Thus, observations made to determine the interval of high water between two stations, situated at the commencement and at the terminus of a straight reach, may show a rapid transit of the wave at that time, although the reach itself may consist of a very intricate, tortuous, and shoal course up to the period when the tide has surmounted or overrun the sand-banks with which the channel is encumbered, during which, or on the first half of the tide, the rate of progress of the tide must necessarily be as slow, as it will be found rapid at or about high water.

The early surveys or charts of the Tyne afford little information of the ancient state of the navigation. Fryer's survey for the Corporation of Newcastle-upon-Tyne, in 1782, contains so few soundings, that it may rather be considered a record of the available depths over the principal shoals than a correct representation of their features, such as are found in later surveys. An earlier chart appears in the work by Captain Collins, the hydrographer to the Admiralty in the reign of William III. It is little better than a sketch; but at that time it appears that the bar had on it 21 feet at high water of spring-tides. There is so much merit in the usual surveys by Collins, that the chart of the Tyne can only be looked upon as a local work, adopted by him from want of time to produce a better by himself, as he tells us when writing about the chart of the neighbouring River Tees.

The survey made by Messrs. Giles, in 1813, for Mr. Rennie, was the first correct representation of the state of the Tyne; but it did not extend beyond the bridge at Newcastle. The remaining tidal course of the Tyne, from Newcastle Bridge to Ryton, was not surveyed until 1849, when a chart of it, upon the same scale as Rennie's below bridge, was made by two of the Author's sons, Messrs. W. E. and C. H. Brooks, on which the existing channel and shoals are closely defined by soundings. And here it is necessary to note, that the practice of taking lines of soundings at regular distances apart, up and down stream, should only be followed where the condition of the channel continues tolerably uniform. Thus, in Rennie's survey, the lines of soundings are at three chains apart from each other; but, although generally close enough, yet it is a fact that the governing shoal, called Tyne main shoal, was situated between two of those lines of soundings, and thus escaped notice. On this account, in the survey above bridge, the lines were taken so as to insure a correct account of the navigation.

In 1813, and up to the Author's first knowledge of the Tyne in 1832, Tyne main shoal had on it only 2 feet at low water of spring-tides, making, with the lift of a datum tide at that place, only 13 feet 6 inches at high water of a high spring-tide. Tyne main shoal was removed previous to 1842; but at that date, Hebburn, the governing shoal, had on it 2 feet at low water of spring-tides, and 14 feet 6 inches at high water. Willington shoal, a mile lower down, had on it only 1 foot 10 inches at low water, and 15 feet at high water; and Howdon shoal 2 feet 6 inches at low water, and 15 feet 6 inches at high water. In 1858 the available depth over the shoalest part of the navigation was not less than 18 feet at high water of spring-tides.

The accompanying charts of the Tyne in 1813 and 1854, Plates 11 and 12, show the progressive state of the navigation, as the works were constructed to scour away the shoals by the action of the current.

The chart of 1813 contains on it the whole of the works recommended by the late Mr. Rennie for the improvement of the Tyne, and the works which had been commenced upon that design by the Author's predecessor, the late Mr. Anderson, a pupil of Telford's.

The chart of 1854 contains the whole of the works below Newcastle Bridge, as designed by the Author, the works executed being shown by continuous black lines, and those in progress by dotted lines. This chart necessarily shows the dock constructed by the North Eastern Railway Company in Jarrow Slake, from the design of Mr. T. E. Harrison (M. Inst. C.E.), and Northumberland Dock, planned by the Author.

The system proposed by Mr. Rennie in 1813 is, in principle, very similar to that which was executed by Golborne on the Clyde, a series of groynes or jetties to contract the navigation where too wide and shoal in its natural condition. These groynes in the Clyde had, at a comparatively small expense, effected an immense improvement in the navigation, and given a stimulus to the trade of Glasgow, which fully justified a further outlay for increasing the depth, by connecting the ends of the groynes by walls parallel to the new course of the channel, and by removing from the latter, by dredging, all hard materials.

During the period of Mr. Rennie's survey of the Tyne in 1813, that Engineer was engaged in carrying into execution the connecting river-walls advised by him for the improvement of the Clyde. Nevertheless such works did not form any portion of the scheme recommended by him for the Tyne, as he was aware of the propriety of deferring the formation of parallel river-walls until, as in the Clyde, their construction would become more easy and economical, by reason of that change in the sites on which they would have eventually to be constructed, by the shoaling which would take place after the formation of the groynes.

Having made the observation, that the natural effect of the construction of groynes is the ultimate conversion of the intervals between them into land, it is necessary to notice that this latter result is only partially fulfilled where, as in the examples of the works in the rivers Dee and Clyde, the channel was at first too violently contracted by the groynes, so that in times of heavy land-floods the latter are dammed back until the stream acquires sufficient head to overflow the groynes, gully out channels below them, and thus remove much of the deposit which had taken place between the groynes while the river was in a less flooded state.

The error of too great a contraction of channel, which had been committed in the Dee, and the Clyde, was avoided by Mr. Rennie, in his plan for the improvement of the Tyne, as well as in that by the Author, which was made in anticipation of the future removal of Newcastle Bridge, and of improvements in the channel above, which would allow the land-floods to have a quicker discharge, and thus insure the establishment of a deeper channel at low water.

While thus apparently disapproving of the excessive contraction of the Clyde by Golborne, and by Rennie also, because the execution of the parallel river-walls of the latter rendered the subsequent widening of the navigation, on the increase of the trade of Glasgow, a matter of necessity, it must be remembered that Golborne had exceedingly limited funds for the improvement of the navigation; and even in Mr. Rennie's time, when he recommended the formation of the parallel or longitudinal walls, the revenue of the port would not have justified the belief, that means would be available for obtaining a wider and deeper navigation by dredging. In Golborne's time dredging machines were not in use, so that he had to depend upon the natural scouring power of the current for the formation of the improved channel proposed by him. The contraction of the Clyde by Golborne's jetties was commenced about 1780, and had the effect of producing a greatly increased navigable depth; and the subsequent steady increase of the revenue of the Clyde fully justified the expenditure upon its channel of large sums, for constant dredging, to maintain a width of channel and depth beyond the natural power of the stream.

Thus, even now, after an expenditure of above a million of money since 1770, in pure river works alone, viz., on groynes, river-walls, and extensive employment of dredging power, it is not to be doubted that the navigable channel would be rendered unavailable for vessels of more than 15-feet draught during spring-tides, if the dredging operations were to cease for even a period of only two years.

When Golborne, in 1768, made his first report on the improvement of the Clyde, the revenue of the port was only £147. In 1780, after his works had been two years in progress, it was only £1,515, while when Telford, in 1806, and Rennie, in 1807, recommended the addition of the parallel river-walls, the revenue only reached to about £8,000. Since that period a great increase of the trade, and an augmentation of the river-dues, have raised the revenue to £111,000.

When the Author first took charge of the improvement of the Tyne, the annual sum allotted by the Corporation of Newcastle for river works was about £5,000, out of which provision had to be made for dredging the berths at the public quays. Economy in the river works, extending over many miles of navigation, was, therefore, a matter of necessity; and when the first report of the

Author was made, in January, 1843, the works then proposed, which consisted solely of timber groynes, or jetties, involved an outlay of only about £18,000. It will suffice, to give an idea of the extent to which river works may be carried out, even with a limited income, to state that the whole of the works in the Tyne, below Newcastle Bridge, consisting of groynes and connecting walls, where necessary to obtain the required depth of channel, were executed during sixteen years, out of an income of certainly less than £5,000, or an expenditure of about £80,000, as contrasted with the outlay of above a million on the Clyde—a smaller river—in similar works, to obtain about the same available navigable depth. There was, however, a great advantage in the construction of the river-walls of the Tyne over those in the Clyde, arising from large quantities of chalk and other materials being able to be procured at a smaller price per ton. The adoption by the Author of timber for the construction of the groynes or jetties was also the means of diminishing greatly their cost, as compared with the groynes of the Clyde, which were principally of rubble. When the Author ceased to act as Engineer to the Conservators of the River Tyne, at the end of the year 1858, the revenue of the port was free from any debt on account of the river works, the whole having been executed out of the annual allowance for river works, with the exception of the pier works and Northumberland Dock, for which separate available sources had been procured by Acts of Parliament for their special construction.

By comparing the original design by the late Mr. Rennie with the plan of the works for the improvement of the navigation above Shields Harbour, it will be seen that the principle upon which Mr. Rennie based his views for the improvement of the Tyne has not been departed from, and that the alterations made by the Author were such as arose from the necessity of taking into consideration the subsequent establishment of important shipping places on the banks of the river.

Thus, in the Long Reach, several collieries had constructed shipping places on the northern shore, without reference to which the Author's predecessor had commenced the formation of a stone groyne at Willington, on the same shore, above them; the consequences were immediately felt by the shoaling-up of the Hay Hole Channel, and the formation of a shoal stretching three-fourths over the channel towards the southern shore. The Author had therefore to adopt remedial measures to turn the current again down the Hay Hole Channel, which was effected by his design of January, 1843. Eventually, to establish dock accommodation for the shipment of the steam coal, the Northumberland Dock was executed upon the Author's design, its southern margin, or quay, being made to form the new northern shore of the river very nearly upon the original line shown on Mr. Rennie's plan. The dock and tidal basin, and other works connected therewith, occupy about 70 acres. of the ancient bed and foreshore of the river, and the access, through the 70-feet gates, to the tidal basin, and thence to the great dock, is so easy that it is not an unusual circumstance to see two brigs towed through the basin into the dock at the rate of 3 miles an hour.

Every variation made by the Author from the original design by Mr. Rennie carries with it its own reason for the same, either on the score of greater economy, or the necessity of providing for established interests; and it is believed such variations have been effected, without losing sight of the great principles upon which all river improvements should be based. In the case of Bill Point, which Mr. Rennie proposed to cut off, at a cost of £16,630, a small portion was removed by the late Mr. Anderson, to the level of low water of equinoctial tides; but the base warped up with sand to a depth of 2 feet, showing that an extensive cutting was requisite, in order to insure a greater current. Many casualties occurring on account of the dangerous navigation round the Point, the Author received powers to carry into effect his own plan. On this work Admiral Washington, the late hydrographer to the Lords of the Admiralty, thus reported[1] to their Secretary on the 15th of November, 1848:—

> "At Bill Point, or rather in St. Anthony's Reach, immediately above it, the ebb-tide was formerly deflected from the convex south shore, and thrown into the deep bight on the north side, just above the Point; in like manner the flood-tide in coming up Bill Reach was thrown off by Bill Point, and forced into the bight on the opposite shore; thus leaving a spit between the two tide-sets known by the name of the Bill Sand, extending over five-sixths of the high-water width of the river, and narrowing the navigable channel at one time, it is said, to 130 feet, while the reaches above and below Bill Point lay nearly at right angles to each other. Mr. Brooks, engineer to the corporation, boldly grappled with this formidable evil, and by planting groynes or jetties from 150 to 250 feet in length completely across the deep-water channel in the bight, he has entirely changed the course of the river, scoured away a large portion of the Bill Sand, and left the mariner a comparatively easy navigable channel, an undeniable benefit to the navigation of the river."

The above description relates to a work which only cost the Conservators £840, and was completed in less than two months, and is an example of what may be executed by the trustees of a navigation who have limited funds at their disposal.

It will be seen that the great principle of improvement, that of easing or enlarging the radius of the curvilinear channel round Bill Point, has been already effected, and that no outlay upon cutting at the Point can practically enlarge the curve, without at the same time advancing the southern, or concave shore. A large expenditure may be made upon Bill Point, but the deep-water course will still be along the southern or concave shore, and the alteration at the Point, if made, will be simply the removal of rock to find the same replaced by a sand-bank; and it is fortunate that such must be the result, as any other would destroy the important shipping station for coal which now occupies the southern shore below the Point.

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[1] *Vide* "Report of the Commissioners appointed to Inquire into the Present State of the River Tyne," &c., p. 468. London, 1855.

At St. Peter's, or rather at Dent's Hole, a deviation was also proposed by the Author, who does not doubt, that had Mr. Rennie been called upon to realize his views, he would have suggested the same modification. The quay was erected, and no one doubts the improvement effected.

It is quite competent for any one to find some defects in the plan of the Tyne works, as realized. Thus, even the Author would have been better satisfied, if he had been enabled to carry out the northern concave shore abreast of Hebburn Point more boldly to the southward, and have avoided the necessity of any works on the latter shore. But Mr. Rennie's line on the north or concave shore had been commenced, and the Coxlodge staiths had also been carried out to the same line; nevertheless the noted Hebburn shoal still remained, and the deepest sailing channel was round Wallsend Bight, so called on account of the Roman wall having its eastern terminus on the shore of that Bight, where one of the Author's groynes is shown on the chart of 1854, (Plate 12.)

The works in this part of the river afford a useful lesson to Engineers in charge of river improvements, as demonstrating that cases may occur where mere local contractions of the navigation may appear to have failed. Thus the quay at Low Walker, constructed by the Author's predecessor, had been undertaken with the view of removing Hebburn shoal, on which as many as half a dozen steamboats, not drawing more than 3 feet 6 inches of water, often remained aground for more than two hours at a time, the depth in the best of the channel, and that a very narrow track, being barely 2 feet 6 inches at low water.

A member of the Newcastle corporation held up this contraction of the navigation as an instance of the folly of interfering with nature. But the Author rejoined that the fault did not rest with the works in question, but in the want of a proper direction being given to the current previous to its entering the contracted navigation, and that, owing to the current being allowed to cross the channel at Hebburn in a diagonal direction, or from quay to quay, in lieu of holding a course between them concentric with the north and south shores, the real width of the moving current was much greater than appeared. Eventually, the Author was allowed to carry out his views; the jetties on the southern shore were extended into the channel above Hebburn, and this, the governing shoal, which had been uselessly dredged again and again, ceased to require further attention.

It is not, however, maintained that dredging is useless. It is necessary where the bed of the river consists of hard clay, or of heavy stony materials, unless a great velocity of current can be made to bear upon it, which in some cases may be attended with inconvenience. Dredging operations will also prove permanently useful, when they are undertaken to aid the current to obtain a depth which the latter could not effect unassisted, although it might have sufficient energy subsequently to keep open the depth when acquired. It must, however, be obvious, that where dredging operations are carried on to a depth and breadth of channel disproportioned to the natural power of the stream, or preponderating influence given by the drainage water, the result must be a deposit in the channel closely following up the progress of the dredger; and no hope can remain for the maintenance of the required depth, except by constant dredging, or by the execution of works above, which may have the effect of accelerating the discharge of the land floods.

A river Engineer should always seek to make the natural power of the stream do the work of deepening the channel. He should use the water power, which costs nothing, in preference to steam power, for which his employers must pay. In the example of the scouring effect of the current on the bed of the Tyne, on which the Author had solely depended, evidence was given before the Royal Commission, in 1854, showing that, even before the longitudinal walls were commenced, the effect produced was at that time to the extent of the removal of "two millions and a half of cubic yards."

Conflicting opinions having been advanced. as to the utility of the drainage of a country in the preservation of harbours, the Author submits that, in all cases where the harbour assumes the form of a deep inlet or channel, having a great length of course and limited breadth, such a channel must necessarily fill up, or be converted into land, unless it be kept open by the admission into it of the drainage of the country, to give a preponderating influence to the ebb of the tidal wave. The only cases in which backwater, or the drainage of a country, can prove injurious, are where the harbour consists of a tidal receptacle or bay, in which the back sweep of the wave, natural declivity of the shore, and bed of the harbour seaward, are of themselves sufficient to insure the preservation of deep water; and also where the tidal harbour consists of an inlet of a bold rocky coast, having such a great depth of water abreast of it as to be unaffected by the waves from on-shore gales. Such a harbour as the latter will preserve its depth even although it may be of a form which prevents its having the benefit of the back-sweep, or retiring wave. It is obvious that the addition of the drainage of a country, or backwater, would be injurious to a harbour of this kind, on account of its producing a stronger outset or run of tide against an on-shore wind, thereby increasing the 'send' of the wave, and rendering the entrance of the harbour more dangerous. In all other cases, nature shows, by the formation of land in estuaries and bays, that no deep inlets can be maintained without the influence of the drainage of the country, and that those inlets become of little value as navigations when the drainage, or backwater, is diverted into numerous channels, such as those by which the drainage of the midland counties, and the fen district of the east coast, finds its passage to sea.

In the case of a river whose channel has been contracted within limits which the natural scour is able to maintain to the required depth, the contraction may be said to have been made to a judicious extent; and a close investigation of the tidal phenomena of such a river will show that, although the superficial area formerly covered at high water may have become less, nevertheless its useful capacity as a tidal reservoir will have been increased.

The depression of the low-water surface produced by the river works has hitherto only taken place, to any great extent, above Shields Harbour; and there remained in 1858 a considerable fall between the west side of the Insand and the bar, sufficient to account for the long stagnation on the latter during the early division of the flood-tide. The description already quoted from the late Admiral Washington's report about the Bill Sand, applies equally to the Insand; but the means for the removal of the latter were not so much at command. Abreast of the Insand is the North Pool, or the site of the berths for large ships. This anchorage was supplied with two lines of mooring chains, secured with Mitchell's screw moorings. As this part of Shields Harbour has a depth of from 20 feet to 30 feet at low water, and is, in fact, the only part of the harbour where vessels of large draught can lay until they can get to sea, it is evident that caution was necessary to be observed; or that no works should be executed which would compromise the depth now enjoyed, until a deep-water dock near the mouth of the harbour afforded compensation. That deep-water dock was planned by the Author in 1845, but met with strong opposition from rival interests. All reasonable grounds of opposition were removed when the pier works were determined upon, and an Act of Parliament was obtained to authorize their formation; but the opposition continued until the Author ceased to be Engineer to the Conservators of the Tyne. The first act of the new Engineer was, however, to recommend the construction of a dock on the same site.

The southern wall of Tynemouth Dock would have given a direction to the tide, in conjunction with the effect of the pier works, which would have led to the improvement of the Narrows, the Insand, and the tail of it, called the Middle Ground. As a temporary measure for scouring away the Middle Ground, and improving the channel at the Narrows, a breastwork was recommended by the Author, to be constructed a little to the westward of the Low Lighthouse, to enclose the recess called Peggy's Hole.

The above reference to unexecuted works is necessary, in order to account for the non-improvement of that part of the harbour which, according to the Author's theory on the cause of the existence of bars, is so essential towards the amelioration of the condition of that of the Tyne.

An investigation into the condition of bar harbours shows, that the depth is not due simply to the quantity of tidal water with which the channel is supplied, or to the amount passing over the bar, but to the manner in which the tidal receptacle receives and discharges the wave, or column of tidal water; so that, in fact, a river with a smaller quantity of tidal water, whose mouth is comparatively unobstructed by the discharge of drainage-water during the natural duration of the flood-tide at sea, will be in a better state, or have more depth on it, than one which may receive double the quantity of tidal-water.

The increased momentum of the tidal column, due to the augmentation of the hydraulic mean depth, has been before noticed; but it is well that attention be also given to the great difference which exists between a river free from shoals, and one which consists of a series of pools, separated by shoals or inner bars. In the former, the momentum is given to the column of tidal water ranging from the bottom of the channel to the surface of the current; while in the latter, or shoal river, it is almost restricted to the column of water ranging between low and high water, or to the simple lift of the tide; the water in the pools remaining comparatively sluggish or inert, and having, in fact, to be dragged along by the tide which passes over the natural weirs or shoals below it. On the ebb-tide the same check to the momentum exists. This consideration of the case is essential to a right understanding of the principle upon which Golborne, Smeaton, Watt, Rennie, and Telford have recommended works for the improvement of tidal navigations by means of lateral contractions.

Much of the controversy on the subject of the inclosure of Jarrow Slake arose from the adoption, by the late Mr. Rennie, of a mistaken notion as to the supposed influence of Jarrow Slake in accelerating the current of the flood-tide over the bar of the Tyne, by reason, it was alleged, of the sudden expanse of the river at that point, or increase of the tidal receptacle, at the time when the tide reached the level of half-tide at sea. The observation is made by Mr. Rennie in his Report, not from any deduction from his own tidal observations, but evidently from its being the generally well-received belief, upon which no doubt had ever existed.

It is also a fact that, even with all the experience of the late Mr. James Walker, he fell into the same error, and advised the opposition of the Admiralty to the Jarrow Dock Bill, brought forward by the North Eastern Railway Company.

In Mr. Rennie's Report on the Tyne, he says:—

> "The width of the river is, however, so very various, that the rate of the current varies in almost every part of it, and is generally the greatest when Jarrow Slake is just covered, which is a little before half-flood. Vessels entering the harbour at this time frequently derive advantage from this great expanse; for if they enter with an adverse wind, the increase of current helps them over the bar, when they otherwise would not be able to enter the harbour at all. This Slake is of great extent, covering upwards of 350 acres, and when the water is covering it, there is an increase in the velocity of the current of nearly one quarter of a mile per hour, an increase of material advantage to the shipping."[1]

If, in this case, Mr. Rennie, instead of admitting as a truism the popular notion, that Jarrow Slake was covered with water when the tide had risen to the level of half-tide on the bar, had examined any one of his own tidal sections, he would have found that at the time of half-tide on the bar the surface of Jarrow Slake was dry throughout, and was actually several feet above the surface of the channel of the river adjoining to it, and that the extra velocity at half-tide on the bar was due to other natural causes, common to all tidal harbours.

Let it then be assumed, for the sake of argument, that the bed of Jarrow Slake, or any such receptacle, lies at a lower level, and that it would be covered at half-tide on the bar, and that the inquiry is then as to its effect upon the flowing tide, in consequence of the sudden enlargement of the tidal receptacle. This subject was investigated by the late John Macgregor, surgeon of the Royal Artillery, who published a pamphlet upon it in 1836; but he, even at that date, was unaware of the fact that Jarrow Slake was dry at the period of half-tide on the bar, and his reasonings are therefore applicable to the case in question, and are given with a perspicuity which it is believed will carry conviction of their soundness. After enunciating some well-known general principles, Mr. Macgregor observes, relative to the transit of the tidal wave, that—

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[1] *Vide* "Papers relating to the River Tyne, ordered to be printed by the River Committee," p. 4, Newcastle, 1836.

"The chief retarding causes are, friction, impact, eddies, and dilatations of the channel, or enlargement of its sections. In regard to the latter cause, we have already seen that the velocity of a stream is diminished in proportion to the enlargement of its bed; and, without authority, we learn, from common observation, that when a swollen river overflows its banks, from that moment its velocity is checked. If we keep this simple principle in view when we contemplate an upward flow, we could not fail to perceive that the same result must follow in both cases, unless we dispute the axiom that the same causes produce the same effects under similar circumstances. In short, all the conditions which modify the descending stream must, from the nature of fluids, modify in the same manner, and, *cæteris paribus*, in the same degree, the upward current."[1]

Admitting, then, that the expansion of the bed abates the velocity of the ebbing waters, it cannot be maintained that the same expansion will have an opposite effect on the flood-tide.

Mr. Macgregor proceeds to remark that, "It is Mr. Smeaton who thus speaks—

"'The tide is spent by many turns in a river, and in filling the loops, and is thereby prevented from rising so high perpendicular, as where the course is straight, and with a more regular contraction.'"[2]

"Further, as a land-stream is not affected by an enlargement in advance until it reaches it, so neither is the tide affected by Jarrow Slake in any way whatever until it arrives there, and the waters begin to diverge in that direction. But at the point of divergence there is a resolution of force into, as it were, two components, to which the antecedent force in Shields harbour may be considered the resultant, or equivalent. Now, as each of the components must be less than the resultant, it is clear that there must be a diminution of the velocity at the point of divergence. Again,—as the rate at which the waters overspread the Slake is at least three times slower than in the channel, this lesser rate cannot accelerate the greater, because it is impossible that a slow motion in advance can accelerate a swifter motion behind. Again—Jarrow Slake has an ascending slope from the margin of the river to the opposite side, where it is, for the greater part, about three feet higher than the river line. As water, therefore, cannot flow against a slope, save by a force behind, it is clearly this force which sends it into the Slake, which has, therefore, no power of *in-draught*, but, on the contrary, opposes the progress of the tide by this additional obstacle of slope."

On the bar of the Tyne half-tide is at about 8 feet below datum, and as the general surface of Jarrow Slake is only between 4 feet and 5 feet below datum, it is evident that even at three-quarters flood on the bar, the tide only begins to ripple over the deepest part of Jarrow Slake.

In the case of the large enclosure of the tidal area at Jarrow Slake, it was not pointed out by Mr. Walker, that the sudden expansion of the river at the Slake was the cause of an equally sudden elevation of the bed of the main channel of the river, which, from having a depth at low water of 20 feet before meeting Jarrow Slake, decreased to less than 18 inches in the main channel abreast of the Slake. The bed of the river at that part consisted of a vast tract of shoal water, having only a narrow deep-water swatchway through Hay Hole on its north side, made by the flood-tide, but which had no deep-water outlet at its upper or western end at Howdon, the depth there being only 2 feet at low water over what was called Howdon Shoal; a mere passage through the shoulder of the great Jarrow sandbank, where the latter abutted on the north shore.

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[1] *Vide* "Papers relating to the River Tyne, ordered to be printed by the River Committee,' p. 32. Newcastle, 1836. [Tract 8vo., vol. 71.]

[2] Smeaton Reports, vol. i.

On referring to the chart (Plate 12) it will be seen, that a straight line through the centre of Shields Harbour runs directly into the Slake, in lieu of up the main channel of the river; so that, in fact, a more injurious feature could not possibly exist as regards the welfare of the navigation.

The tide setting directly into this Slake, could only reach the main channel by falling into it laterally, or similar to passing over a weir, leaving on the flood a strong eddy, or counter-tide along the southern or Jarrow quay shore above the Slake.

The examples of land reclamation from such places as Jarrow Slake, or from the beds of shallow rivers, must not be confounded with those which are made out of pure tidal receptacles, or from harbours of nearly a pure tidal character, such as Milford Haven or Portsmouth Harbour. In such tidal harbours the alterations which take place by gradual accretions of land are exceedingly slow, and centuries pass with scarcely any visible alteration in their form and depth.

With regard to the reclamation of lands subject to tidal overflowings, the supposed interference with nature has been sufficient to cause the rejection of measures for the enclosure of tidal mud lands which could not possibly have proved injurious to the interests of navigation, while they would undoubtedly have added to the power of producing food, and were therefore of national importance.

Lastly, with reference to the enclosure of loops, or lateral tidal receptacles, it is not doubted that the tidal water which enters them would be useful in maintaining the channel below, provided the body of water were thrown into it, without injurious effects taking place at the same time upon the main navigation or channel. Thus, if in lieu of the Jarrow Slake expansion, the same number of cubic yards could be thrown up another channel, which would possess a communication with the main channel without injuriously affecting it, then, in that case, advantage would undoubtedly be gained by increased depth in the channel below, just as the Stour, on its uniting with the Orwell, forms the deep-water harbour of Harwich, below the junction of those streams.

Abreast of Jarrow Slake the range of tide from extreme low water to high water of spring-tides was only 13 feet 6 inches, as compared with a 16-feet tide at the mouth of the river. But at first of flood on the bar the difference was much greater, the tide not having ebbed out to within 12 inches of the level of the river at low water; so that there existed a fall of 3 feet 6 inches between Jarrow and the bar, in a river course of less than 3½ miles, or a slope seaward of 12 inches per mile antagonistic to the young flood then making on the bar. This conflict between the flood-tide and the pent-up back-water is readily seen at the mouth of the Tyne. A float, nearly of the same specific gravity as sea water, if placed in the sea abreast of Sharpness Point, will be found to gradually settle down through the lighter stratum of water which it encounters on approaching the bar, until it is upborne by the denser stratum of salt water below, which is forcing its way into the river. It appears, also, by an examination of the tidal sections, that the flood current does not attain power to turn back the ebbing waters until a slope or head is established of between 2 inches and 3 inches per mile.

The river works, executed by the Author, between Newcastle and Shields, consist of groynes or jetties formed of yellow pine timber, braced together with waling pieces of half-balks, and the spaces between the main piles closed up with 3-inch sheet-piling. The average cost per running foot of the jetties was about twenty-five shillings. One advantage of this mode of effecting material changes in a navigation is the greater certainty of rapidly carrying out the views of the Engineer, than where earth or stone is used. As nearly the whole of the works were constructed upon shifting sands, care was necessary to prevent the occurrence of scouring action by the tides, in the line or on the sites of the groynes; and much cost of construction was saved by delivering from barges, on to the site of the work, enough clay or chalk rubbish to form a stratum of about 9 inches in thickness over the surface of the sand, for about 3 yards on each side of the line of the groyne. Another precaution observed was, wherever practicable, the closing up, in the first instance, of the deepest parts of the section, or where the rapidity of the current was the greatest.

The great changes in the channel were produced by the groynes, as in the Clyde, unaided by connecting walls, which latter, following the practice in the Clyde, were formed of rubble, chalk, and scoria, or slag from iron furnaces, after the sites upon which they had to be constructed were shoaled up many feet by the eddies produced by the jetties.

At the termination of the Author's engagement with the Tyne Improvement Commissioners, at the end of the year 1858, the whole of the groynes and connecting river-walls had been completed between Newcastle and Shields. The only portions of the river lines then incomplete consisted of certain advanced quays at Tyne Main and St. Anthony's, where, however, there was little prospect of the works being executed for many years, if at all, on account of the old quays being held on short leases, or too uncertain tenures to justify the expense of rebuilding them on the new lines. These works are also of minor importance, the depth in the present navigation abreast of them being sufficiently good to enable the Commissioners to safely leave the reconstruction of the quays to their owners.

In the tidal navigation above Newcastle Bridge but few works were executed under the direction of the Author. They consisted solely of the enclosure of a bight above Paradise Quay, the deepening of a rocky ford below Lemington, and the removal or deepening of the channel at Blaydon and Stella, by the construction of quays and partially closing the channel on the north side of Blaydon Island, the effect of which was an immediate lowering of the low-water surface of the river at Stella to the extent of 18 inches, and the consequent more rapid discharge of the land-floods.

The above, and the construction of many hundred yards of public and private quays, constitute the Tyne river works above Shields Harbour.

The Northumberland Dock, in Jarrow Reach, and the Tyne Dock works, in Jarrow Slake, have also materially contributed to the improvement of the navigation of the river.

SHIELDS HARBOUR.

This reach of the Tyne is about $1\frac{3}{4}$ mile in length, and consists of two deep-water pools, or natural docks, that on the south having a depth varying between 15 feet and 20 feet at low water of spring-tides, and that on the north side having a depth varying between 18 feet and 25 feet.

Both these pools, in which frequently seven hundred colliers and other ships lie moored afloat, were provided with Mitchell's screw-moorings, and of their utility it is enough to say, that it is not an unusual circumstance for above one hundred and fifty sail to slip away and get safely to sea in an hour.

With the exception of the construction of a few quays in unison with those above and below, but slight prospect existed of effecting any material improvement of the Shields reach of the river. The shores on both sides, already occupied by quays, and in general covered with buildings to the margin of the river, insured the perpetuation of the circuitous navigation of Shields Harbour, with its deep-water pools and attendant shoal ground abreast of them, with the necessary consequence of a loss of tidal range by the imperfect discharge of the backwater. The Author followed the example of Mr. Rennie, in giving a design for cutting off a portion of White Hill Point; but he coupled with it the caution, that the attendant result would be the diminution of the depth of water in the mooring-berths of the harbour, so that it would not be a judicious measure until efficient dock accommodation were provided. He showed that, as a matter of science, it was necessary to recommend the removal of the Point, but that practically there existed, by the presence of heavy tiers of ships in other parts of the harbour, much greater obstruction to the tidal currents than at the point in question, and that the material works for removing the greatest obstacles to the free entrance of the tidal wave would have to be constructed at the Narrows and below.

A small breastwork was proposed by the Author, to close up the bight immediately above the Low Lighthouse at Shields, with the view of producing a scouring action upon the tail of the Insand, known by the name of the Middle Ground. Means were also adopted to obtain a greater depth on the Middle Ground by harrowing, on the failure to obtain consent to enclose Peggy's Hole.

As the Insand is the site of an eddy made by the current of the ebb on leaving the South Shields pool for that at North Shields, it must be evident, that the effect of any dredging operations upon the Insand must be only temporary, and attended by an equally temporary diminution of the depth in the pool abreast of North Shields.

SEA REACH AND BAR OF THE RIVER TYNE.

The works for the improvement of the Bar and entrance to the River Tyne form a subject for discussion of themselves, and might doubtless throw light on the cause of the existence of bars, whether they are met with in tidal or in tideless seas.

That the subject has not received the general attention which the vast interests connected with it deserve, is evident from the crudeness of the ideas which have often been propounded, and the timidity with which they have been usually expressed.

It is said by some professional men "that each river requires its own peculiar treatment," as if nature departed from the laws by which all matter is governed, or as if the same causes which create shoals or bars in rivers like the Tyne ceased to be applicable to the Indus or the Tagus.

The Author has already shown, that he attributes the existence of the bar of the Tyne, like that which exists at every bar river where the tidal influence is felt, to the circumstance of a confliction of the early flood tide with the latter part of the current of the ebb out of the river. It was not, therefore, to be expected that he would recommend the projection of piers into the sea, with the hope of thereby getting rid of the bar of the Tyne, inasmuch as however far they might reasonably be extended, they would not have the effect of materially reducing the level of the bed of the sea-reach, or that of its surface at low water. All the benefit which piers could produce would, therefore, be limited to the effect of a judicious concentration of the power of the currents of the flood and ebb tide, and to the safety and facility they would afford to ships entering and leaving the port.

The late Mr. Rennie proposed, in 1813, the construction of the large south pier on the Herd Sand. To this pier the Author objected, on the ground that during the most dangerous gales from the north-north-east round to the east, it would be similar to a rocky reef to leeward of ships trying to enter the harbour, and upon which they would be drifted by both the wind and the tide; and also that the pier would be as injurious in conducting seas into the harbour, as it would be ineffective in carrying out the object for which it was proposed—that of facilitating the entrance of the tide.

As a preferable measure the Author proposed, in the year 1845, the northern pier, originally planned by him on a visit to the Tyne in 1832, in connection with a sea dock on the foreshore between the Low Lighthouse, North Shields, and Tynemouth, in the excavation of which dock material would be obtained to assist in the formation of the pier. Subsequently, the Author proposed the addition of a southern pier on the Herd Sand, which would have enclosed a large area available for the site of a south dock. The Author's northern pier, with slight deviations, was adopted in designs by the late hydrographer, Admiral Washington, by Staff Commander E. K. Calver, R.N., an experienced marine surveyor, and by the late Thomas John Taylor, all of whom were well acquainted with the Tyne. So far as regards giving a proper direction to the effluent water, it would undoubtedly have been more efficient in securing a greater amount of depth of water on the bar, than by the plan already completed for about two-thirds of its intended length, which has rendered the port comparatively safe to run to in on-shore gales at a proper time of the tide. This last plan of piers was preferred by the Author, because ships after passing between the pier heads would be at once out of danger; because it would allow an immediate subsidence of the incoming waves, in lieu of conducting them up the harbour; and because vessels, entering with a scant wind, would be able to luff up after passing the pier heads, and then sail more freely up the sea-reach. Mr. Walker's design, now in course of execution, is almost identical, the only material difference being the reduction of the width between the pier heads from 1,400 feet to 1,100 feet, a contraction which will, in the Author's opinion, have the effect of forcing the bar into a dangerous position seaward of the pier heads, where the shore tidal current has not a greater velocity than about one knot per hour.

In the year 1850 application was made to Parliament to obtain powers for constructing piers at the mouth of the Tyne and other works. Mr. Rendel recommended a southern pier, which may be described as a combination of the outer division of Mr. Rennie's pier of 1816 with the inner division of the Author's southern pier. In the following year, Mr. Rendel suggested the addition of a small pier on the northern shore, but not extending so far seaward as either of the north piers previously proposed by the Author. Mr. Rendel's view with regard to the north pier appears to have been, that it was desirable to keep the pier head and bar as much as possible under the shelter of the headland of Tynemouth; a safe position, if it could have been carried out, and opposed to the notions afterwards entertained, when the scheme was propounded for converting the bar harbour of the Tyne into a harbour of refuge, by the extension of the piers into a depth of 36 feet at low water of spring-tides. This, it was stated, would be the means of getting rid of the bar of the Tyne, which ordinarily has only about 6 feet at low water, and in its best condition only 8 feet.

The Tyne piers were commenced, and the foundation plate bears the name of the Author as the Engineer. When nearly two-thirds advanced towards completion, the desire of claiming for the Tyne national funds, to convert it into the required Harbour of Refuge, so much wanted on the north-east coast, having been expressed, the late Mr. Walker gave the design, extending the original piers into a depth of 36 feet at low water. It was thus gravely proposed to change the Tyne from a third-rate port, according to its natural features, into a first-rate port, such as the Humber or the Thames.

The survey of 1854 may be considered to represent the state of the navigation when contracted by the groynes, which had been constructed between Newcastle and the west end of Jarrow Quay. The works in the Long Reach below were subsequently executed, and the river-wall was only formed on the north side for its full extent in 1858, towards the close of the Author's professional engagement. Many years must elapse before the full benefit of these works in deepening the navigation can be said to have been attained, or after the action of many powerful land-floods.

Any permanent improvement of the bar of the Tyne will be found to result from the concentration of the energy of the current of the ebb-tide upon it, much of which is now wasted by passing seaward over the rocky foreshore at the mouth of the harbour. The present dredging on the site of the bar merely removes sand, which the current would of itself effect, as the channel becomes contracted by the extension of the piers.

Lastly, as regards the proper form of the piers, so far as respects their influence upon the oceanic current or tide:—It has been assumed that it would be better to have the southern pier extended further seaward than the northern pier, in order to catch or accumulate the tidal wave against it.

In reply to this view, the Author calls attention to the south pier, as planned by Mr. Rennie in 1816, evidently with this object, and certainly carrying in its favour the general opinion of the public on both banks of the Tyne, as the Author found when he first propounded entirely opposite views.

Rennie's south pier-head would really be westward, or within the headlands projecting into the sea on the north side of the river. These headlands have the effect of a northern pier in deflecting the current, so that the main body of the tide sweeps round and enters the Tyne from east and from south-east, a strong flood-tide actually setting into the harbour from south-east to north-west over the Herd Sand. This sand forms the south shore of the sea-reach, and is thus "alive on the flood-tide," which latter sweeps into the harbour from off the Herd Sand every tide many thousand cubic yards of sand, which are again deposited on the Herd, while the site of the latter is in an eddied state during the whole of the ebb.

It is therefore obvious that as the south pier could not possibly have any influence in changing the direction given to the oceanic current by Sharpness Point, it would, on the contrary, exist as an obstruction to that division of the current which now enters from the south-east.

It is assumed, at all events, that a certain amount of tidal wave would be accumulated against the northern face of any pier on the Herd Sand, which has a direction similar to that proposed by Mr. Rennie, from the action upon it of the shore tidal current. But it must be remembered that the latter is very weak, and the effect produced would be almost insignificant. Its result would not be one-hundredth of that which is caused by the accumulation of the tidal wave by the bold projection of the southern coast of Tees Bay, where the lift of tide is only 16 feet, as contrasted with a tide of 15 feet on the same day at the mouth of the Tyne. Although the principle of forming the mouth of the receiver of a funnel shape is good, it is only practicable in those cases where it can be carried out unattended with the injurious effect of exposing the harbour to the inroad of heavy seas. It will be enough to provide a sufficient width of channel between the pier-heads, and increase the hydraulic mean depth of the channel within, and a safe reliance may then be placed upon the receiver being filled by the *vis à tergo* of the oceanic wave, or the statical pressure outside the piers.

On this subject the late Mr. Walker's evidence was, in the Author's opinion, as sound as it was the reverse when speaking of getting rid of the bar by the extension of the pier-heads.

With reference to the Tyne piers, nothing could be more clear than the evidence of the late Mr. Robert Stephenson, who, on being asked—4155. "Your attention has not been called to the cases upon this coast in which you have, so to speak, a south pier as a projecting headland rather than a north pier?" replied, "If it is a case of navigation, which I think must always be kept in view when you deal with piers projecting out in that way, you must consider how a ship can be manœuvred. Suppose she is coming in under sail from a north-east wind in distress, I think you ought to have the north pier there to the windward; the long pier I think ought to be to the windward."

This advantage of the protrusion of the northern pier beyond the southern one is already greatly felt, and ships are now enabled to sail much more readily into the Tyne during heavy weather from the most dangerous gales, which are those from north-east to east. There is also the advantage of the present site of the bar of the Tyne receiving shelter from the north pier, thereby causing a diminution of the 'send' of the sea on the bar; this advantage will be lost if the Tyne pier-heads are brought so near to each other as has been suggested; inasmuch as the bar would then be seaward of the pier-heads, and the harbour would be more dangerous to make, as the oceanic current does not exceed one knot an hour, and is certainly too weak to remove the sand which would lodge there, owing to the conflict between the flood and ebb tides. That the late Mr. Robert Stephenson fully coincided with the Author on this view will appear by the following question, and his reply before the Royal Commission, on alluding to the conflict between the ebb tide out of the Tyne and the early flood tide. 4238. "Then at that time, the water being quiescent within that space, whatever silt that water contains, whatever it may have brought down in suspension, will, probably, according to the general nature of such matters, be more or less deposited? That I believe to be the true cause of the bar."

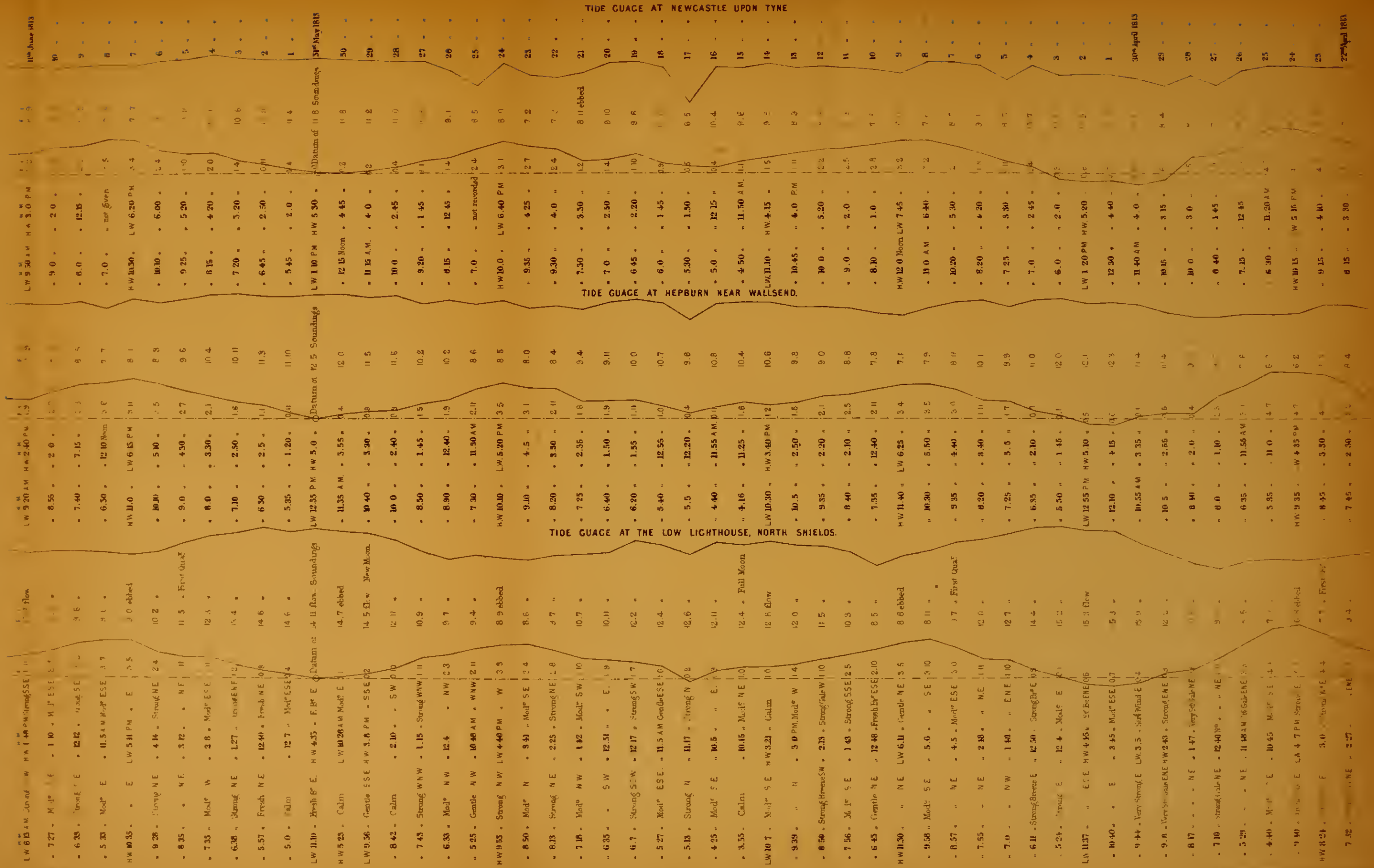
The observations in this Paper have been mainly directed to a description of the works below Tyne Bridge; for although the Author has partially executed some in the 7½ miles of tidal navigation above bridge, he willingly leaves the harvest of reputation to be gathered there by his successor, Mr. Ure, who furnished plans for its improvement in a Report of the 29th of October, 1860, which involve an expenditure of £384,000.

Numerous plans and sections were lent by the Author to illustrate the Paper, from which Plates 10^{B}, 10^{C}, 10^{D}, 11 and 12 have been compiled.

[Mr. W. A. Brooks

RIVER TYNE.

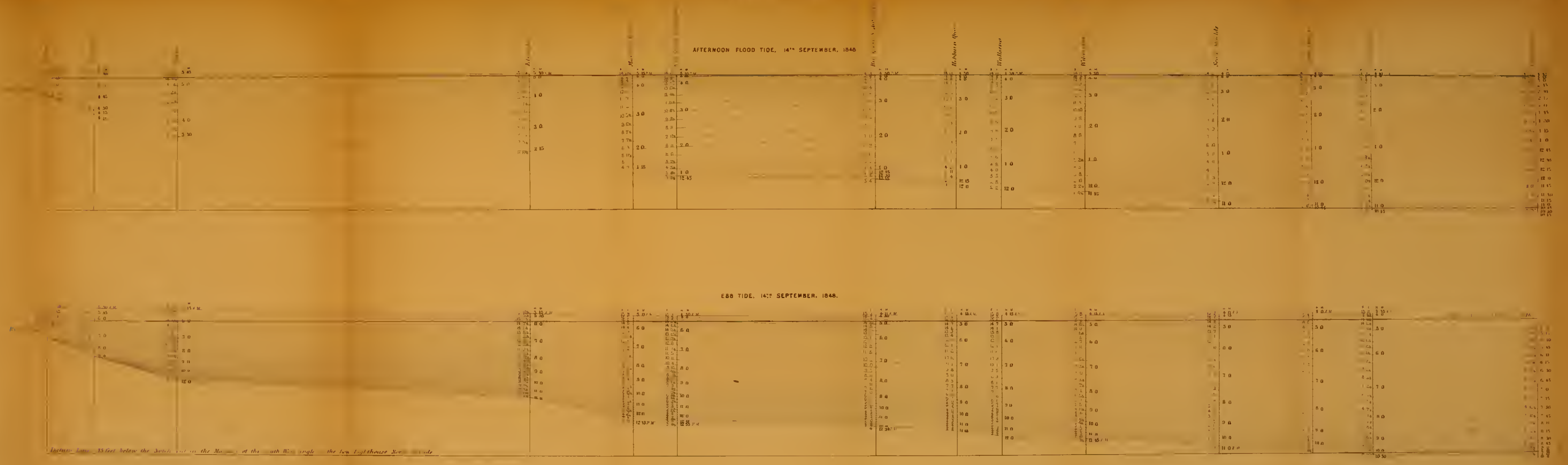
FROM THE TIDAL OBSERVATIONS MADE FOR Mr RENNIE IN 1813.



W. A. BR[illegible]KS DEL^T

[illegible]

RIVER TYNE.



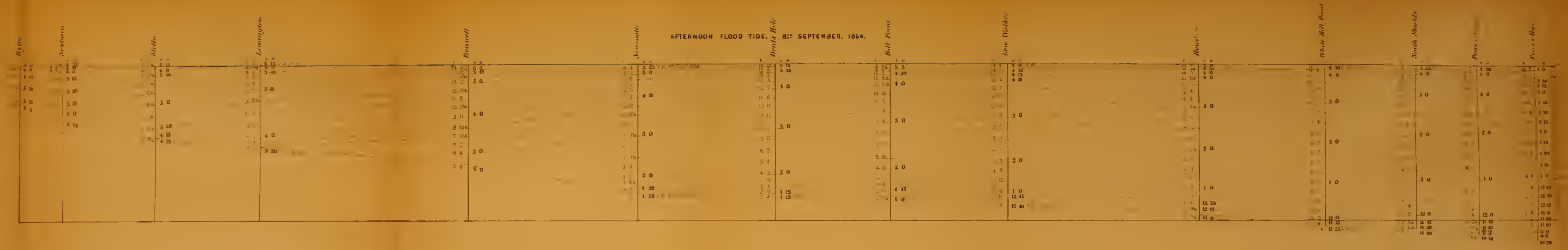
AFTERNOON FLOOD TIDE, 14TH SEPTEMBER, 1848.

EBB TIDE, 14TH SEPTEMBER, 1848.

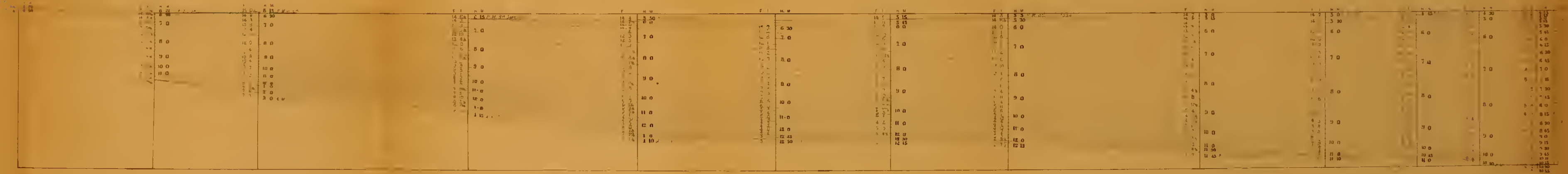
Horizontal Scale 1 Inch to 1 Mile
Vertical Scale ⅛th of an Inch to 1 Foot

RIVER TYNE.

AFTERNOON FLOOD TIDE, 8th SEPTEMBER, 1854.



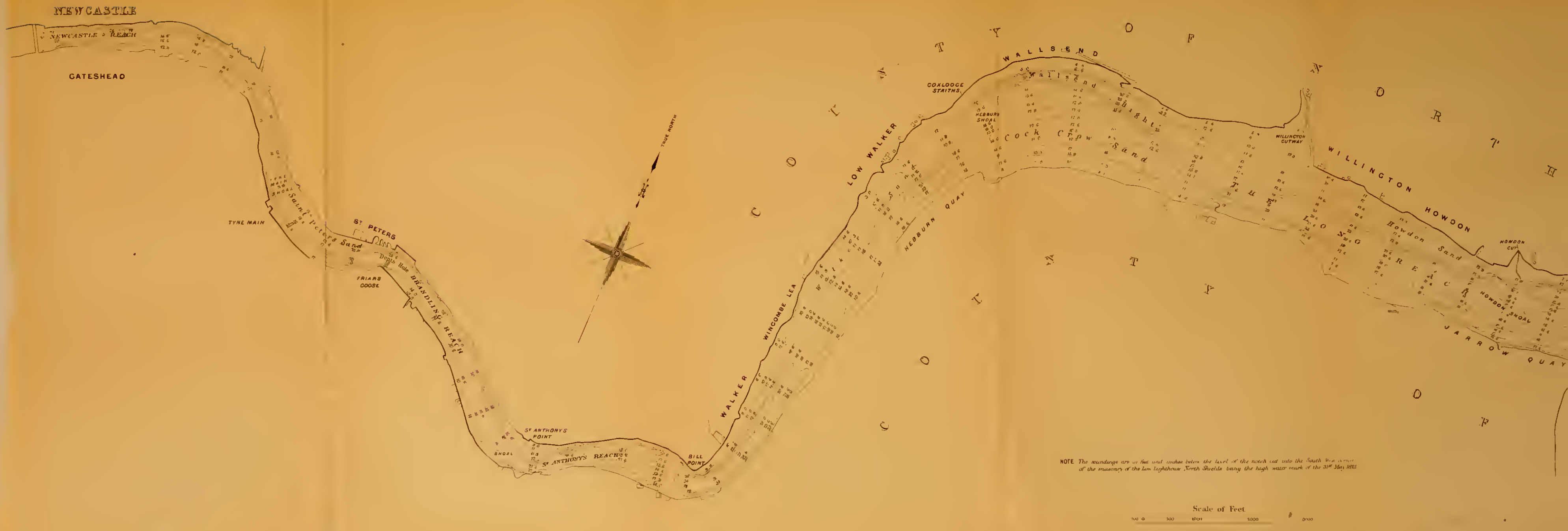
EBB TIDE. 8th SEPTEMBER, 1854.



Horizontal Scale 1 Inch to 1 Mile.
Vertical Scale ⅛th of an Inch to 1 Foot

RIVER TYNE.

SHOWING THE STATE OF THE NAVIGATION IN THE YEAR 1813.



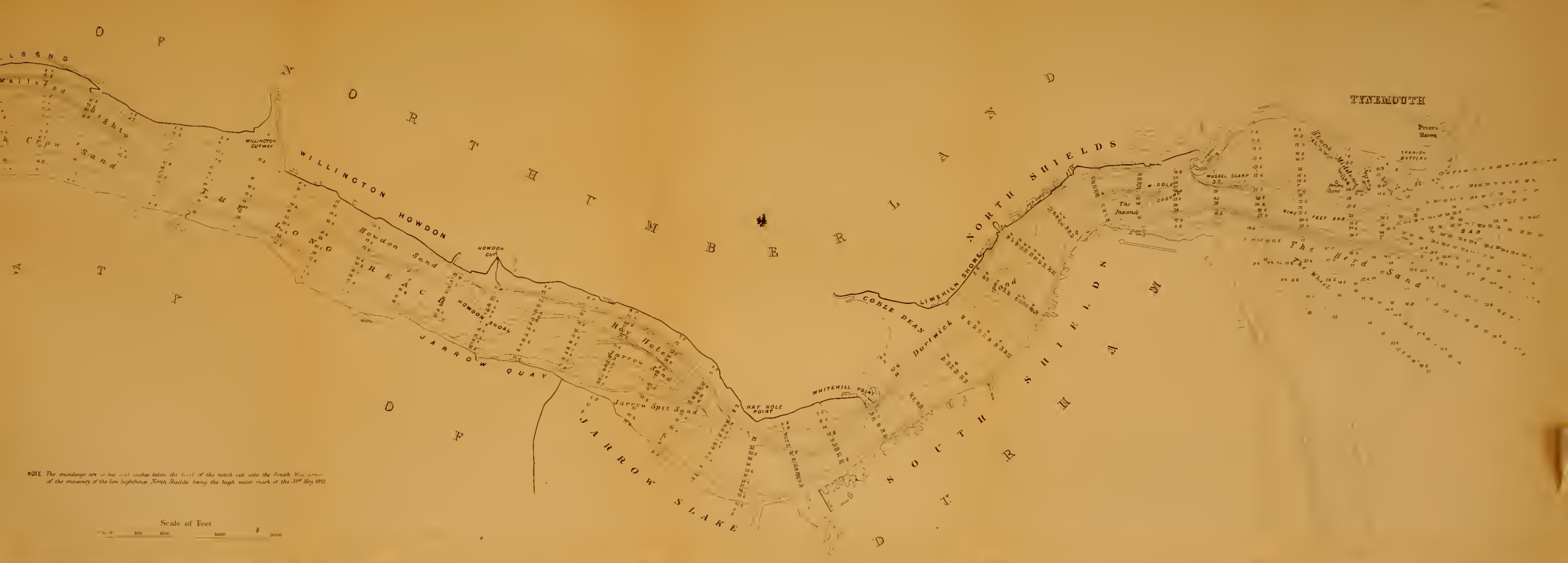
NOTE. The soundings are in feet and inches below the level of the notch cut into the South West corner of the masonry of the Low lighthouse North Shields being the high water mark of the 31st May 1813.

Scale of Feet

500 0 500 1000 2000 3000

RIVER TYNE.

SHOWING THE STATE OF THE NAVIGATION IN THE YEAR 18.3.



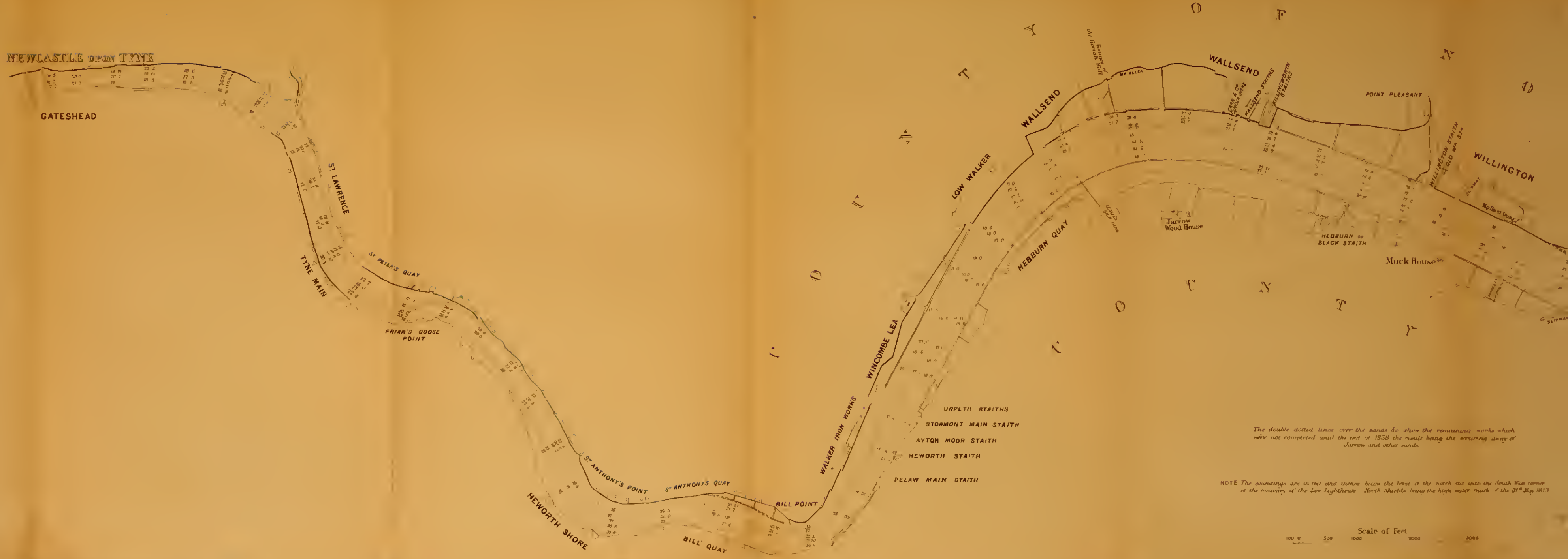
NOTE *The soundings are in feet and inches below the level of the notch cut into the South West corner of the masonry of the Low lighthouse North Shields being the high water mark of the 31st May 1813*

M[illegible] V. XX[illegible] Session 1860,6[illegible]

KELL BROS LITHRS [illegible] LONDON

RIVER TYNE.

SHOWING THE IMPROVEMENTS EFFECTED IN THE NAVIGATION UP TO THE AUTUMN OF 1854



The double dotted lines over the sands &c show the remaining works which were not completed until the end of 1858 the result being the scouring away of Jarrow and other sands.

NOTE *The soundings are in feet and inches below the level of the notch cut into the South West corner of the masonry of the Low Lighthouse North Shields being the high water mark of the 31st May 1813*

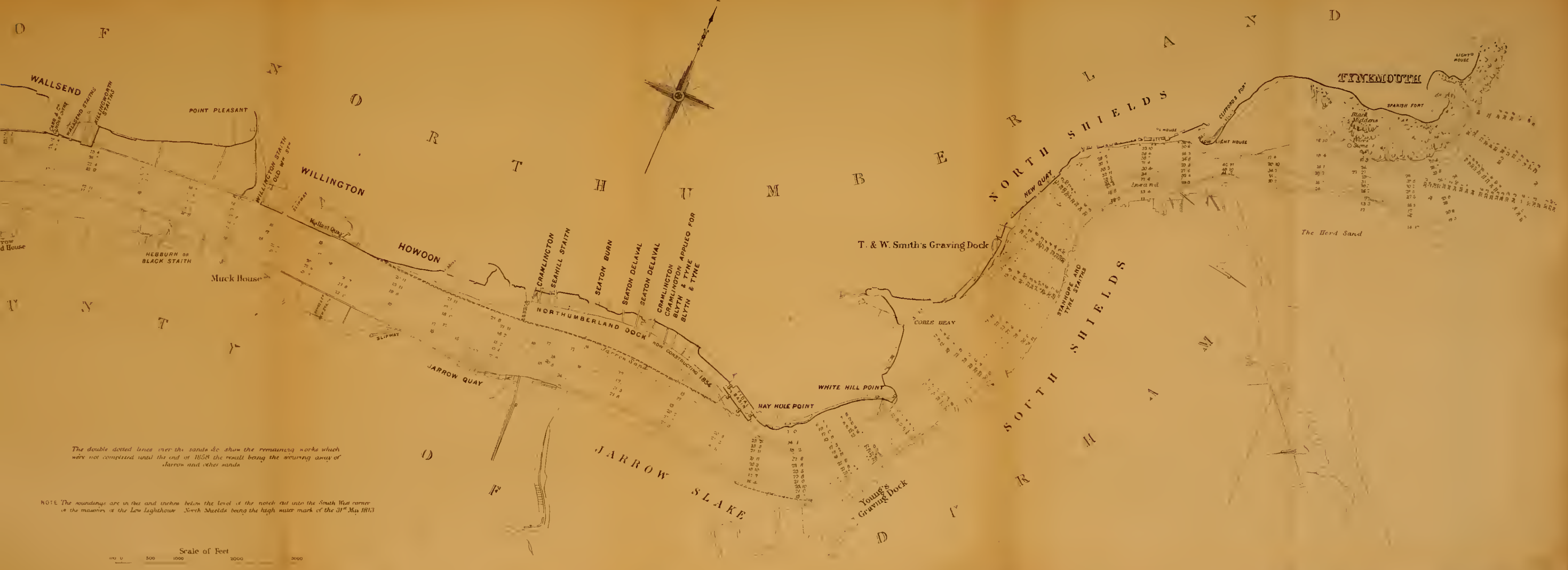
Scale of Feet

100 0 500 1000 2000 3000

Minutes of Proceedings of The Institution of Civil Engineers

RIVER TYNE.

SHOWING THE IMPROVEMENTS EFFECTED IN THE NAVIGATION UP TO THE AUTUMN OF 1854



The double dotted lines over the sands &c show the remaining works which were not completed until the end of 1858 the result being the scouring away of Jarrow and other sands

NOTE *The soundings are in feet and inches below the level of the notch cut into the South West corner of the masonry of the Low Lighthouse North Shields being the high water mark of the 31st May 1813*

Scale of Feet
100 0 500 1000 2000 3000

Minutes of Proceedings of The Institution of Civil Engineers [illegible] Session 1866-67

KELL BROS LITHRS CASTLE ST HOLBORN

Mr. W. A. Brooks said the Paper contained nothing as to the present state of the River Tyne. Mr. Ure, his successor, had made extensive use of dredging vessels, and there was no doubt the river was improved at the present day. On the bar there was now a depth of 17 feet of water at low water of spring-tides, where in the best times there was formerly only 8 feet; but that had been accomplished by a large expenditure of dredging power,—principally in Shields Harbour and on the bar. So long as this extensive dredging was continued on the bar, and so long as powerful dredging machines were able successfully to combat the operations of nature, so long the public might be congratulated upon having 17 feet of water on the bar instead of the small depth he had stated, although that did not suffice to make the Tyne a bonâ fide refuge habour. He had always held, that it was impossible to bring the Tyne into a state to make it a harbour like the Thames or the Humber, with a depth of 30 feet at low water. It was stated in evidence before the Royal Commission on Harbours of Refuge, that by carrying out the Tyne piers to a depth of 30 feet at low water, that depth at low water would be permanent.

He thought it was desirable that the Institution should possess a true record of the number of tons raised from the bar of the Tyne since the year 1859, when Mr. Ure commenced the dredging operations. If a calculation of the cubical contents on the bar were made, taking it from the inside to the outside below the level of low water of spring tides, and it was contrasted with the same measurement at the present time, it would show a displacement to the extent of about 360,000 tons. That was the entire alteration which had been produced after dredging for so many years. It was only necessary to contrast that quantity with the number of tons raised to form an estimate of the probability of the extent of duration of the present depth on the bar. So long as dredging was vigorously continued this depth would be maintained. The number of tons raised during the last year was about 5¼ millons. From 1850 to 1858, only 1,135,000 tons were raised, while his successor, since the latter date, Christmas, 1858, had raised no less than 20,254,000 tons. The result, adding 10 per cent. for the use of the plant, gave an expenditure for the year of £100,835, being at the rate of 4½*d.* per ton for each ton raised in 1866, 20 cubic feet being reckoned as one ton. The whole of the material raised was discharged from barges into the sea. Some of the screw-hopper barges cost as much as £6,000 each, and they were able to do the work very cheaply.

Adverting to the subject of bars, a remark was made in the Paper as to the want of proper attention to it on the part of Engineers. The late Mr. James Walker said of the Tyne; "That all the outgoing water, being confined by piers, instead of spreading over so wide space as at present, would, at the ends of the piers, join the coast tide passing from south to north, with increased velocity or energy. . . . For the position of the bar is by no means fixed; it is at the point where the opposing forces, west to east, and south to north, meet or balance."[1] Mr. Walker's theory was that the cause of the bar was the opposition from the current going from west to east and that of another going from south to north; whereas, instead of opposing each other, it was clear they went all together. There were two forces, and the resultant must be diagonal between the two. His theory of the formation of the bar was this: viz., the real conflict which took place when the flood tide came in and met the as yet undischarged water of the ebb. Very often a rise of nearly 2 feet at sea took place before the tide turned at all into the harbour. The late Mr. Robert Stephenson coincided with him on this point, and he hoped this would elicit the opinions of Engineers on the subject, as nothing was more important than the getting rid of bars in rivers, if it were possible to do so. In a lecture he had delivered at the Royal United Service Institution, "On the Cause of the Formation of Bars at the Mouths of Rivers," he had stated:[2]—

"A very common theory which has long been advanced, is that the primary cause of bars, is "the transverse motion of the wave and current crossing the debouching waters of the river," and the Thames is cited as an example of a river in which there is no bar, because its mouth is freely open to the tidal current, and there no transverse action upon it takes place.

"If this theory were correct, all bars could be removed by simply turning the direction of the seaward reach, to make it freely open to the tidal current, or to insure that the flood current, and that of the ebb, should travel over the same course.

"Nine out of ten who profess to be able to give an opinion on this interesting subject, the bars of rivers, hold this view of the case, and it appears plausible enough, but it is not borne out by reference to facts; thus in the case of the River Tees, of which the Author had charge for many years as engineer, its sea-reach or mouth, between the north and south Gare sands, is usually freely open to the tidal current, yet that river has a bad bar, although it discharges the drainage of a large district, and consists also of a capacious tidal receptacle.

"The direction of the sea-reach of the Tees varies as much as ten points of the compass, and when occasionally open to the east, and then having its debouching waters crossed by the tide, it certainly is not shoaler than when the sea-reach bears from south-south-west to north-north-east, or with its mouth freely open to the current of the flood-tide.

The bar of the Tees is due to the presence of numerous inner shoals of sand, and a ridge of rock in the circuitous channel through the estuary of the Tees, which have the effect of penning back the drainage water of the country, until long after the tide has begun to rise on the sea bar."

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[1] *Vide* "River Tyne Improvement. Piers at Entrance." The Report of James Walker, 15 November, 1853. Westminster. (Tract 8vo., vol. 95.)

[2] *Vide* "Journal of the Royal United Service Institution," vol. vii., p. 190. London, 1864.

He would not trespass longer upon the Meeting; but he wished to draw attention to this subject, believing it to be one of the most interesting that could be brought under the consideration of this Institution.

Mr. Gregory, V.P., thought most Engineers, instead of adopting the theory of the formation of bars to which Mr. Brooks had alluded as erroneous, took a more simple view, viz., that the material held in suspension by water travelling at a certain velocity would fall to the bottom and form a deposit when that velocity was checked; and this view was consistent with the explanations given of the observations at the mouth of the Tyne. The incoming tide, when it met the water discharged by the river, would check the velocity of this water, and so cause deposit, which formed the bar.

Mr. Vignoles thought it unnecessary to travel into recondite experiments and observations as to the causes of the formation of bars. Mr. Brooks had laid down what no doubt might be taken as the general principles. Where the flood waters met the incoming flow of the tide there must necessarily be a deposit. Indeed that rule might be extended considerably farther. It was well known that the same occurred where the Mississippi fell into the Gulf of Mexico, the waters of which were comparatively still, the amount of tide being small. It was so also at the mouths of the Danube, issuing through its delta to the Black Sea, which was tideless. It was sufficient that the outflowing stream of the river should be met by the vis inertiæ or resisting medium of a large body of water, and there inevitably the bar must be formed. He thought Mr. Brooks had shown a great deal of ingenuity, and a great amount of practical knowledge, in the way in which he had operated upon the various points and reaches of the Tyne. He had shown a good example, and had judiciously ceased to endeavour to force, by means of an artificial current given to the river, what nature would not allow to be done. Mr. Brooks' successor had struggled against nature, and, by a large expenditure of money in dredging, had succeeded in obtaining a considerable depth of water on the bar. No doubt where nature had placed a river on which man had brought an artificial traffic, in order to maintain the means of carrying on the trade created, artificial resources must be employed upon it, and therefore it was that Engineers were engaged in the work of fighting, as it were, against nature. Nature had created rivers, and wherever there was a river there was also a bar; and no doubt if harbours could be had where there were no rivers they would be better than where there was a river; but in this and all other civilized countries trade sought for itself shelter within a river, consequently the efforts of man were directed towards overcoming the obstructions of nature. Thereupon arose the employment and the skill of the Engineer. He thought

Mr. Brooks had successfully shown what might be done with the interior of a river; but artificial resources would be necessary at the mouth. He did not know what had been the final results of Sir Charles Hartley's operations in the Danube; but he understood that the bar of that river had been improved at first. He would repeat that wherever the great current of a river met the sea—whether it was with an incoming tidal current or mere dead waters, like those of the Black Sea or the Gulf of Mexico—there the bar must be deposited; and if more than the natural depth of water which the bar afforded were wanted, recourse must be had to the artificial means of dredging, and this could only be kept up at a large annual expenditure. It was a subject of interest to know to what extent the interior of the River Tyne had been improved, and also to hear that Mr. Brooks' successor continued, *à force d'argent*, to maintain this greatly increased depth of water on the bar.

Mr. Bateman wished to ask whether any effects, independent of the dredging, had been produced upon the bar by reason of the extension of the piers.

Mr. Brooks replied that that was just the point to be proved, and time alone would do so. When there was no more money to spend, then the theory would be tested and the truth found out. His successor had taken away the sand bodily, and when he raised 5 millions of tons a year, it must make a considerable hole. At present, the flood tide came in almost immediately, and the level of low water must have gone down a foot or eighteen inches.

Mr. Bateman had asked this question in order to draw attention to the fact, that the effect of the construction of the North Bull Wall upon the bar at Dublin Harbour had been eminently successful. By the action of the current concentrated on that point, the bar had been deepened from about 6 feet to 14 feet or 15 feet at low water, and that was effected before dredging to any material extent had been carried on. He regarded this as a most successful engineering operation, and he believed that success led very much to the adoption by Mr. Walker of the particular form of entrance of the piers erected in the Tyne, and it would be interesting to know whether the same natural result had followed in that river as was obtained in the case of Dublin Bay.

Mr. Vignoles believed that the success, in the case referred to by Mr. Bateman, was mainly due to its being accompanied by dredging at the same time. The stream of the Liffey, which was weak, had been able to maintain it when executed: although he believed the result was not entirely from natural scour.

Mr. Bateman believed that dredging was carried out to a moderate extent in the upper part of the river, but that the bar itself was operated upon almost entirely by the current, so far as he was informed, without the aid of dredging on the bar. Whether the dredging above had any effect upon the natural scour, he could not say.

Mr. Hemans remarked that the effect of the North Bull Wall had been very successful in increasing the scour on the bar; but the depth of water could not be kept up without some dredging, which was carried on to a moderate extent. The natural scour did an immense deal, and had been most successful.

Mr. G F. Lyster observed that the Paper was so diffuse that he was not prepared to enter upon the discussion of it. With regard to the Mersey, where, as was well known, a bar existed, the depth of water over it had been fully maintained, care being taken to interfere as little as possible with the capacity of the river, so as to admit of its freely receiving and expelling a large body of tidal water; the leading channels of the Mersey had therefore remained in the same condition for the last fourteen or fifteen years. Careful soundings and surveys of the river and the bar were made every year. The natural channel had maintained a depth of 12 feet at low water of spring-tides, and there had been no works lately undertaken within the river which interfered, to any important degree, with its full water capacity and discharge. The abstraction of water by the construction of the Birkenhead Docks had had no effect upon the bar of the Mersey; although at one time it was thought that the loss of so considerable an amount of water as that from Wallasey Creek would affect the condition of the entrance channels to the river, but the depth over the bar remained the same as it was fourteen or fifteen years ago.

Mr. Bateman remarked that two excellent surveys of the Mersey had been made—one by Mr. Giles, and another by Mr. Robertson Wright. The former having been made twenty years ago, and the latter since the construction of the Birkenhead Docks, it would be seen on a comparison of the two sections, that the sectional area of the estuary above Liverpool had been increased from 5 to 10 per cent., which would more than compensate for the abstraction of the water of Wallasey Creek, which had been alluded to. By the straightening of the walls on the Liverpool side as well as on the Birkenhead side, and by giving a true run to the current, the scour was so much improved, that the estuary was now larger in capacity than it was before the Birkenhead Docks were constructed.

Mr. G. R. Stephenson said, although his acquaintance with the Tyne commenced from his earliest days, having been born on its banks, his avocations had called him for the greater portion of his life away from that locality; still his visits to it had been frequent, and having noticed what was going on, he was tolerably well acquainted with the general character of the works that had been carried out during many years; though he had no doubt Mr. Harrison, who had constructed the Jarrow Docks, was in possession of details and figures of which he had not command. Having had considerable experience in dealing with the drainage of fen countries in England and abroad, the opinions he would express on this subject were founded upon that experience.

He would remark that the first practical step, towards the question of dealing with the Tyne, was taken when the construction of the Tyne Docks was proposed by Mr. Harrison. Previous to that, he believed,. the subject had never been fairly opened out. The question that then arose was, whether the abstraction from the river of the quantity of water which occupied the space of the Jarrow Slake would be detrimental or otherwise to the tidal flow in the Tyne? That was discussed by some of the most eminent practical men of the day, many of whom had now passed away; and he believed, with the exception of the Admiralty authorities, the opinion was almost unanimous, that the walling in of the Jarrow Slake would be of considerable service both to the navigation and to the scour of the Tyne. He maintained that it had been a great advantage; he also thought that if the Jarrow Slake wall were continued further, the effect would be to drive the water higher up the river between Jarrow Slake and Newcastle Bridge, and that, when the old Tyne Bridge was taken away, it would have the effect of giving the water a very rapid communication between the top of high water—at Newcastle, say—and the sea. The object was to get the water to run down to the sea as rapidly as possible; and if the tide were drawn so much higher up the river than at present, it was clear the water must be carried down with greater velocity. He maintained that the taking away of the old bridge would admit of the water being squeezed higher up the river, and would also admit of the same water being thrown back and brought down again with greater velocity, and consequently greater scour. On the Nene Navigation, by taking away a bridge in a similar situation to that on the Tyne, the low-water mark was reduced 6 feet at its site: in other words, the tidal low-water level of the sea was carried 2 miles higher up the river than before. The bridge removed was Sutton Bridge, and it had been a great benefit to the navigation, as well as to the drainage. Having examined the original plans of Mr. Brooks, as well as those of the works now being carried on by Mr. Ure, he felt that great credit was due to the former for what he had done. With regard to the harbour piers, that portion of the work appeared to have been but slightly changed from the designs of Mr. Brooks, who was therefore entitled to credit or otherwise with regard to the disposition of the piers. But at the same time he must say he did not approve of the position of the piers.

He agreed with all that had been done in the Tyne with regard to dredging and walling, but he thought the operations had been begun at the wrong end.

The question that arose with reference to the Tyne was simply this: When the time came—which he thought must arrive sooner or later—when the dredging would have to be discontinued, would the present state of the river at the entrance admit sufficient tidal water to keep the river in the same state as it was now brought to by dredging? He thought not: he believed if the Tyne Commissioners had expended some portion of the money in taking away the Narrows, a great deal of the dredging, which had to be done over and over again on the same spots, would have been saved, because it would have admitted a larger volume of water up the river, to be brought down again and act as a scour on the ebb. He thought that was clear from the fact, that at the entrance of the river there was a difference of level between the inside and outside of the Narrows; the low water in the harbour being above that of the sea. It was therefore evident that there was a stricture at the Narrows, which did not admit the full extent of the tide. It would have been better to have commenced at the bottom, or outlet of the river, and then to have worked upwards, inasmuch as by that means a large amount of dredging would have been saved, which it was now necessary to do over and over again. He would state generally his belief that the ordinary lift of the tide at the outside of any river had little to do with the quantity of water thrown up it. If the actual lift of the tide at the outside of a river was the sole power that filled up a river, it would be impossible to account for the phenomenon which took place at Chepstow, where the tide rose 40 feet or 50 feet above the level of the sea. It was unquestionable that the great tidal wave coming from the south, and entering the Bristol Channel at a velocity of 70 miles or 100 miles an hour, was projected up the Severn, causing the great lift of tide. With a bell-mouthed entrance to a river (and the Bristol Channel was more than 100 miles across), a larger quantity of water must necessarily enter and again recede, and act as scour.

He did not agree with Mr. Brooks' views as to the piers at the mouth of the Tyne. But before leaving the question of the river, he would mention that his experience led him to use entirely different measures than making groynes at right angles to the course of a river. He was satisfied that the proper way of dealing with silt was to form longitudinal walls at once. It would have been useless to have attempted to put piles into the silt of the Nene or the Ouse to make groynes; therefore he adopted longitudinal walls made with fascines, and the result was very successful. With reference to the piers at the entrance, he felt some hesitation in giving an opinion, because so many reports had been written and plans prepared with regard to them. He would simply deal with the design now being carried out, and he ventured to say in his opinion the design was entirely wrong. That opinion had been entertained by him during the whole progress of the works, and for this reason:—The piers had been in course of construction for some time, and were approaching completion; yet there was no decrease in the number of wrecks that took place at the entrance of the harbour. There had been the most serious mishaps to shipping there, within his own knowledge, during the last year or two, though there certainly ought to be some reliable security to vessels entering the port, but that was far from being the case in the Tyne in anything exceeding a smart breeze. In heavy gales, particularly from the east, the danger to shipping entering the port was very great. A vessel in a heavy gale might have been running 50 or 60 miles trying to get into this port. Owing to the gale, she would carry only the smallest amount of canvas possible—probably reefed topsails, and nothing more: she perhaps could not set anything else outside, as any after canvas would bring her round to the wind, and she would be driven broadside on to the seas, and perhaps be swamped. If the gale were from the north-east, she would have to shave the north pier very closely, and, before she could tack and set her after canvas, she would be nearly to leeward of the inner part of the Herd Sand. If she ported her helm, with no after canvas upon her, she could not fetch to windward of the Herd. He therefore submitted that the north pier for the north-east gales was wrong. Then there was, from the situation of the south pier, a source of danger, in south-east gales, of mishap from the Black Middens. The vessel with little sail upon her had to shave the south pier, which was so far to leeward that with no after canvas she could not weather the Black Middens. There would be no time, especially in heavy broken water, to put on after canvas in that short distance. A clear case of the danger of this was afforded in the instance of the wreck of the 'Stanley.' That vessel was running right in, when, by a slight mishap of the helm, a sea struck her on the starboard quarter, turned her round, and she struck on the Black Middens. In the case of a gale from the east, a sea, striking a vessel on the port quarter, would cause the bow to veer round to the port side, when the vessel would be in great danger of running on to the Herd Sand; but if she were struck on the starboard quarter, her head would be driven starboard, and she would probably go on to the Black Middens. In conclusion, he would remark that a harbour could not be considered as properly designed, in which the greatest danger the mariner met with was after he had got inside the piers.

Mr. T. E. Harrison said, having been asked by the President what effect the enclosure of Jarrow Slake, by the construction of the Tyne docks had had on the river, he did not think he was in position to give any exact answer, for the simple reason, that since the period at which Mr. Brooks left the office which he so long held on the Tyne, extensive works on the river had been carried out under a totally different system. He would incidentally remark, that during the period Mr. Brooks held the office of Engineer to the River Tyne Commissioners, in all the operations he had to carry out, he was limited by the amount which was placed at his disposal for expenditure. Therefore, anything he had done must be judged of entirely by the amount of the funds at his command. Since that period Mr. Ure, who succeeded Mr. Brooks, had had charge of these works; and under a totally different arrangement of the corporate body who now managed the affairs of the Tyne, they, by sundry Acts of Parliament, had been enabled to raise several hundred thousand pounds for river improvements. That had altered entirely the principle on which the works of the Tyne had been since carried out; and any effect which the enclosure of Jarrow Slake might have had upon the river was so insignificant, compared with the extent of the works which the Commissioners of the Tyne had executed, that it was impossible for any one to say what the relative effect of that enclosure might be. Inasmuch, however, as it was some little period after the enclosure of Jarrow Slake before any of those large works were carried out, he believed, that so far from having had any injurious effect, that enclosure had been decidedly beneficial; but the extent to which any facts could be adduced on the subject was so limited, that he did not think it was safe to base any opinions upon them. In discussing this question at the present moment, the difficulty he felt was simply this—that when he constructed the docks at Jarrow Slake, he thought he was going a long way in laying the cills of the dock gates 6 feet below the level of the best water that then existed up to the entrance of the docks; but at this moment, a vessel could enter the river drawing from 8 feet to 10 feet more water than there was over the dock cills. That had been effected by operations of such magnitude, and so different from those which Mr. Brooks had been able to carry out with the means at his disposal, that Mr. Harrison felt, if they were discussing now the state of the River Tyne, they would be doing so in a most imperfect manner. Therefore, he should much prefer, on the question generally, to wait for the Paper which Mr. Ure had promised to lay before the Institution, describing the operations he had carried out, and the results.

Mr. Longridge, as an old Tyneside man, would offer one or two remarks on this subject. He had known this river intimately from boyhood, when there was not more than 1 foot 10 inches of water at low tide in the channel between Newcastle Bridge and Shields. That was previous to Mr. Brooks entering upon the works. He remembered the whole of that gentleman's operations, and it was only right to state, that when he left the Tyne, instead of 1 foot 10 inches of water there were 5 feet or 6 feet over the worst places throughout that length. Mr. Brooks' idea—which was no doubt correct—was as much as possible to turn the ebb and flood tides into the same channel, in order that the flood tide should not meet the ebb and cause shoals. He, however, did not agree with the way in which Mr. Brooks attempted to effect this. He thought the groynes at right angles to the shore were not so efficient as longitudinal training walls; but, however that might be, with the small amount of money which Mr. Brooks had at his disposal, much good had been accomplished. The point to which he would more particularly advert, was as to the operations carried on since the period when Mr. Brooks left, more especially the dredging operations. He was surprised to learn the enormous amount of dredging which had been going on within the last seven or eight years; and he had examined the charts to ascertain what the actual effects had been between White Hill Point, which was the first bend of the river, and the bar. He found, taking the state of the river in 1859, that to excavate a deep-water channel from that point through the bar, to give a depth of 20 feet at low water, and a width of 420 feet at the bottom, with slopes of 1 in 12, which was the most the river would admit, the amount of material to be removed would be, from the bar to the Narrows, 2,340,000 tons, and from the Narrows to White Hill Point 3,065,000 tons, or a total of 5,405,000 tons, or just about the amount that was dredged last year. He found that from 1859 to 1866 there had been lifted from that portion of the river something like 19½ millions of tons; consequently, assuming that there was a depth of 20 feet and a bottom width of 420 feet, which however was not the case, there had been from 14 millions to 15 millions of tons raised which had been filled in again. Taking the cost of dredging even at the low figure at which it was done in this case, it amounted to £33,000 a-year. He asked whether it was possible for a port like the Tyne to support anything like that expense? If that quantity of material had been dredged out, no doubt the river must have brought the material down again. It would not do to lose sight of the fact, that, however the course of the river might have been improved, yet that a river was the great leveller of nature, and would bring down a large amount of deposit, which must be got quit of in some way or other, and to bring that to an embouchure like the Tyne, would be a sure means of causing it to deposit there. Therefore, it was quite hopeless to expect to maintain the present depth of water, except at an enormous expenditure in dredging. He did not say there were not circumstances which might justify such an expenditure; but he thought in the case of the Tyne, if the theory which had been asserted were true, which was, that when the piers were carried out they would keep the river clear from deposit, the dredging should have been delayed. His opinion was, that the piers would not have the effect of maintaining the present depth of water, but that a bar would form outside. He agreed that the most important thing to be done was to open the Narrows; but if that had been done before some inner works had been completed, it would have admitted the run of the sea into the harbour, and have made the shipping places quite untenable in bad weather.

Mr. Cubitt, V.P., remarked, that it seemed to be considered, that the large amount of material, which had been dredged up, had been brought down and deposited by the river. He was not acquainted with the Tyne, but knew other harbours on the east coast—for instance, Yarmouth and Lowestoft—the bars at the mouths of which were evidently composed of sand and shingle, brought by the action of the waves and current of the sea along the coast, in a far larger proportion than of material deposited by the river.

Mr. Longridge observed that the littoral current of the Tyne was very small, and not more than one knot per hour; so that the bar could not be caused by the littoral current.

Mr. Phipps said, the Paper was an excellent record of the condition of the River Tyne during the time that river was under Mr. Brooks' charge, and was no doubt a good illustration of what might be achieved by a moderate annual outlay, when directed by a regular system.

Respecting the Author's favourite theory concerning the formation of the bars of rivers, Mr. Phipps thought that whilst the general notion was correct, that the formation of a bar was due to the fact of the velocity of the ebb tide being stopped wholly or partially over the site of the bar, and hence the deposit of the matter held in suspension by the water, that it was fallacious to attribute this to the extent done by the Author, to the non-emptying from the river at each ebb of a certain quantity of water lying above the level of low water. The argument was, that the conflict of this water with that of the young flood tide caused quiescence, and hence that the deposit took place almost entirely at this time.

Now, if that were so, the time of deposit would be limited to a very short period, and the bar would only in that time receive the deposit of a small portion of the ebbing water; whereas, in his opinion, the whole of the water discharging itself during the ebb tide tended to produce the deposit on the bar. He thought the whole quantity was, more or less, brought to rest by contact with the water outside, according to the state of the ebb tide; and that without this, the quantity of water deposited would be insufficient to produce the effects really experienced.

He had calculated, from the longitudinal section of the flood tide given in the Paper, and on the assumption that 200 yards was the average width of the river,[1] that the volume of water flowing in and

	Cubic Yards.
out of the river in each spring tide amounted to .	13,591,600
and for the portion lying over Jarrow Slake .	2,500,000
Total . . .	16,091,600

Taking the width over the bar as 440 yards, with an average depth of 16 feet during the whole of the ebb tide, the average speed of the ebb tide would be a little under 8 inches per second, or less than half a mile per hour, being a movement sufficiently slow to allow of the deposit of the principal portion of the matter held in suspension by the above enormous quantity of water.

Mr. BEARDMORE said, regarding this Paper as an historical record of the Tyne, he thought its interest would have been enhanced by a more copious introduction of the dates at which the various works had been carried out. The system of works therein described mainly terminated in 1858, and the subsequent efforts involved a vast amount of dredging and other expenditure of capital. On examining a section of the Tyne, and considering the deepening at the mouth, with the rapid slope that would obtain in the fair-way of the river upwards, it appeared inevitable that a permanent deep channel would be effected. Admitting the gigantic amount of dredging, and considering that within the piers a vast extent of the Herd Sand would drift into the cutting, in addition to that which must follow down the river from above Jarrow, it was not surprising that the work should to some extent have to be done over and over again, and prove expensive. But, looking fairly at the section and the form of the new piers, he thought the doubts that had been thrown upon the final success of the operation would be a good deal modified. He considered that 12 to 1 was a far steeper slope than was consistent with a state of rest for sands under these conditions. The strictures on the piers and on the works at the mouth of the Tyne must not be taken as applicable to the complete design, because the heads of the piers were as yet incomplete, and they formed an important element in the design. At all events, a vessel making the harbour without those outer piers must be more exposed to risk. He thought the argument, as to the opening of the Narrows, answered itself. If that had been done without the piers, it would have exposed the most vulnerable part of the Tyne harbour, viz., the Narrows at Shields. The permanency of the new deep-river channel was an interesting question; but it was essentially distinct from the state of the works up to the year 1858, when Mr. Brooks had improved the channel, as a navigation after half tide, in an admirable manner, considering the small funds at his disposal; as might be seen by comparing the original and subsequent survey. But it was to be borne in mind, that vessels since that time had greatly increased in tonnage and in draught of water; then the draught did not exceed 14 feet or 15 feet, and no expedition was made with the freights, and the river was used entirely according to the state of the tide. Now, however, the greater freights were carried in fine ships or steamers of 1,000 or 2,000 tons burthen, which were expected to run in whether the tide was high or low, were unloaded, turned round and reloaded, and started on the return voyage in a few hours. Under these circumstances, he denied that any plan of walls or groynes would be serviceable, unless dredging machines were also used. To show what could be done, by one operation and the other, he would refer to the Clyde, where it had been suggested that a million had been spent in dredging, when groynes would have done the work. In the natural state of the Clyde in 1758, at high water of spring tides, the depth of water was not more than 4 feet as far as Glasgow. Between 1758 and 1824 the river was deepened to from 12 feet to 14 feet, chiefly by the use of groynes and the removal of fords. Up to the latter date not much dredging, if any, had been done by steam; but by means of groynes, and the judicious introduction of connecting walls, so as to turn the groyne system into that of training walls, the vast amount of sand that had been deposited from an early geological period was got rid of. But from the year 1824, when a vessel drawing 12 feet to 14 feet could be taken up to Glasgow at spring tides, dredging machines had to be employed to cut through the boulder clay, and in some cases through solid rock, with the assistance of gunpowder and divers; and the river had been brought to its present depth of 20 feet to 23 feet, at which it was maintained without much trouble, although at the cost of washing down a great deal of Glasgow filth at the lower reaches of the river. He was an advocate for groynes, training walls, and piers, judiciously applied, especially where much sand had to be removed. But where accommodation was required for a large class of ships and steamers, running to staiths or into docks, at all times of the tide, he said—use the dredging machine. In the River Thames there had been a similar application of the dredging machine since 1824; masses of hard gravel and deposits had been removed, and a depth of water had been obtained which it would have been quite out of the power of a current of water alone to effect. The whole of this mass of sand in the River Tyne had been collected within its own area, so that, geologically speaking, the mass was small, considering the ages during which it had been collecting. The harbour there was not like those of Norfolk, Suffolk, or Dorsetshire, where mountains of chalk and drift had fallen into the sea, and formed the banks of which their bars were a part. But in this case there was a cleft between rocky hills filled up by sand, and the bar was the result of the passing currents, acting on the sand deposited within its own limits. With regard to the bar of the Mersey, the great bulk was the result of the destruction of a large area of land on its own site, and the material was not brought down the country as in this case; and there were several other instances of the same class. There were few rivers whose mouths and bars were precisely of the same character; but observation would generally show whence their bars had been derived. In this case his opinion was, that the sand was merely the deposit from the banks and mountains above for many ages; and considering the time it had been collecting, what rapid changes and improvements had taken place, and the fact that the dredging machine had made in seven years a very sensible reduction, he thought the results must be regarded as satisfactory.

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xviii., p. 521.

Mr. Brooks said he should be glad to have Mr. Beardmore's opinion, as to the probable effect that would be produced by carrying the piers into 5 fathoms or 6 fathoms of water; whether he expected that would be a permanent depth, or whether continuous dredging would be required to maintain it?

Mr. Beardmore thought when the dredging had been carried on during a period sufficient to bring the slopes of the channel to a state of rest, it would be a perfect operation. But though a channel were now successfully dredged, yet, taking into consideration the immense quantities of moveable and drifting sands that bordered upon this channel, he thought it would take a considerable time to dredge it to such a condition that the sand would no longer drift. But where this was the case the channel would maintain itself; and it would then be reduced to the state of a harbour without a bar, and with a deep entrance of rock, like Dartmouth, where there was plenty of sand in the upper part of the river, but none at the mouth. The number of wrecks in the Tyne had not increased in proportion to the increased number of ships which entered that port.

Mr. Graham said, that when in 1849 the late Mr. Rendel was called upon to make a report on the Tyne, he assisted the late Mr. Comrie in making for Mr. Rendel extensive tidal observations, and repeating the sections made by the former in 1813, when assistant to the late Mr. Giles.

This survey was most carefully carried out, but did not show any great improvement in the condition of the river; and it was found necessary to call in the aid of Mr. Heppel, to show by figures what could not be shown by plans. Largely engaged as Mr. Comrie had been on the Tyne surveys, the mention of his name ought not to be omitted.

In 1855 a Royal Commission was appointed to inquire into the state of the River Tyne. They sat at Newcastle, heard evidence, and issued, what he thought, a most able and moderate report to the effect, that though there had not been much improvement made between 1813 and 1849, the Commissioners thought no harm had been done, and that the works which were then projected should be carried out, and an opportunity afforded for completing them. The report wound up by expressing a sincere hope, that the differences and disputes which had hitherto prevailed might henceforth cease to interfere with the work of improvement.

He surveyed all that had been done up to 1848, about six years after Mr. Brooks went there, and he surveyed again for Mr. Ure, his successor, in 1859. There had been some difficulty on a portion of the Northumberland shore, in keeping up the navigation to ten or twelve coal shipping places situated at the bend of the river at that place, during the time that the work was being carried out and the course of the river changed; but this was overcome by making, what was now called, the Northumberland Dock. The question throughout was not to make land, but to get a river wall to confine the river; cross jetties or groynes need not therefore have been used. Room would eventually be wanting for the deposit of ballast, as at Cardiff, and the difficulty after a year or two would be to know what to do with the material dredged from the river. His opinion was, that Mr. Brooks ought not to have extended the work over so many years, because everybody was agreed about the course which the river should be brought to. The difficulty was to get the parties to spend more than £5,000 a year. All that had been done was comprised within a distance of 3 miles.

The completion of the Northumberland Dock and the dock and river works on Jarrow Slake had no influence on the river above. The result of the survey of 1860 showed that, since the Jarrow Docks had been completed, no change in the shoaling had taken place for good or bad within the walls.

Mr. J. B. Redman said, he did not possess the advantage of some of the previous speakers, in having any great personal knowledge of the Tyne; but he had visited it twice, in 1853 and again in 1855, when he was struck with the enormous trade of that river, ranking as it did, in commercial importance, as the third port of the United Kingdom in respect of tonnage, and as the sixth port as regarded declared value of exports, being only preceded in that respect by Liverpool, London, Hull, Glasgow, and Southampton. The Author had urged, as a prime objection against the construction of extensive works approximating those which were designed for harbours of refuge, that the Tyne was not, as compared with London or Liverpool, a first-class port. But if reference were made to the Parliamentary Papers annually published of the trade of the country, it would be found that, instead of taking such a position as the Author assumed for it, it was, on the contrary, one of the first ports of the kingdom, coming only after Liverpool and London, the tonnages, export and import, of those ports being respectively as follows:—

	Tons.
London	6,273,951
Liverpool	5,276,648
Newcastle	2,694,657

As regarded geographical condition, the Tyne was a river about 35 miles long, from the junction of the North and South Tyne to the Narrows at North Shields; its two greatest tributaries being the North Tyne, which rose on the borders of, and in the north-west part of, Northumberland, and the South Tyne, which rose 7 miles south from Aldstone, in Cumberland. It was a river which, owing to the elevation of its sources, was subject to sudden and great freshes, and at these times immense quantities of water were brought down. The outfall of the river was discharged from a comparatively narrow mouth, when speaking of it as a port, as compared with London and Liverpool, situated on large tidal estuaries, where there was free range of tide, and that effect was shown in the Tyne in a remarkable manner. The flood tide setting almost north and south along the coast, and the outfall water running east and west direct, the natural result had been the formation of a bar; and the difficulty that the tide had in getting into the river was shown by the fact that outside the bar there was a greater range of tide by 18 inches at spring tides than at the Lighthouse, and the difference of range at neap tides was still more remarkable. An observation had been made by the Author as to the influence of the natural indentation of the Jarrow Slake upon the low water régime of the river. Without attempting to combat the views as to the unimportance of abstracting that quantity of tidal water, the Author argued that that indentation had a bad effect upon the river, inasmuch as it diverted the course of tide that way, and caused the shallowing of the river above; but the same shoaling recurred higher up the river without any attendant indentation like the Jarrow Slake. The Author had also referred to the opinion which had been expressed by the late Mr. James Walker, as to the cause of the bar. On this point Mr. Walker had said, in a report dated the 26th November, 1858, "The position of the bar is by no means fixed; it is at the point where the opposing forces, west to east, and south to north, meet or balance, and it would be moved out beyond the piers if this balancing point were moved out by increasing the current between them."[1]

Reference had been made, in the course of the discussion, to the effect of the operations carried out in Dublin Bay, upon which there was some difference of opinion, although there ought not to be, and he believed was not, as to the facts of the case. The depth of the water had been increased 6 feet; for whereas, formerly, there were only 6 feet of water over the bar at low water, there were now 12 feet, and 24 feet at high water of spring tides. That had not been accomplished by means of the pier alone, but by reason of a large amount of dredging from the bridge at the end of Sackville Street down to the lighthouse, which had cost about £2,000 a year. In that harbour a result had been produced which had not been previously referred to, viz., that though there was double the depth over the bar, and though the harbour was now available for a class of vessels which it could not accommodate before, yet, attendant upon that greatly increased depth there had been a corresponding shoaling beyond the bar. He had lately visited the Danube and the works at the Sulina mouth, and had passed between Sir Charles Hartley's piers in a steamer at sunset; and though he could not state the results from personal observation, from local evidence the condition of the outfall was good. The Admiralty Survey stated, however, that though a considerable increase of depth had been obtained over the bar, yet that a corresponding shoaling and shifting of the bar beyond the piers might be expected. This was an interesting point in reference to the outfall of the Tyne, and the ultimate effect of the piers at the entrance of that river. He believed one argument in favour of the piers urged by the projectors was the testimony of the North Sea pilots, that the bar was good or bad according as there were one or two channels over it. In other words the bar was always good when there was only one channel, and bad when there were two channels. Mr. Walker, in his report in 1858, alluded to what he termed: "The sympathy between the bar and Sparrowhawk or Herd Sands," *i.e.*, when the bar was good or low, the marginal sands were high, and vice versâ, when bad or high the other sands were low. This had been brought forward as an argument in favour of the piers. That was what the Admiralty Chart stated, viz., that when the stream was divided, the bar was bad; but when it was concentrated upon the bar which was the operation contemplated by the construction of the piers, the condition of the bar was good.

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[1] *Vide* "Report of the Commissioners on Harbours of Refuge," vol. ii., p. 818. London, 1859.

Mr. JAMES BRUNLEES said, as his knowledge of the Tyne was but very general, he would only offer a remark or two on the position of the piers. He understood that the object in placing the piers at an angle to the river was to gain water space behind them, and thus obtain a greater volume to scour the bar. It appeared to him that piers so placed would act as sand-traps, and that in time the whole area inside would silt up, and the river course form a straight line. He was, therefore, of opinion that the piers might as well from the first have been constructed on lines more nearly parallel to the river course.

In his experience of forming half-tide weirs in sandy bays he found, that in attempting to run out such works at an angle to the run of the rivers and tides, he had frequently as much as 22 feet of scour below low water; while wherever the works ran parallel to the rivers and tides the greatest scour was only 9 feet, and this latter depth required only about a fourth of the material to form the weirs.

Mr. ABERNETHY said, having been for many years acquainted with the Tyne, and having paid considerable attention to the subject of the piers at its entrance, as well as to the question of the general improvement of the river, he would remark, first, with regard to the projected piers, it was obvious that in their present state they afforded no adequate protection from the effects of easterly seas, the width between the piers being so much greater than that of the channel within. That accounted for the heavy sea and the number of vessels driven upon the Black Middens or the Herd Sand. But it was equally obvious, that when the piers were carried out to the full extent, as he hoped would be the case, then the width between the pier-heads being so much less than the width within, the wave entering would be depressed, and the water within rendered smooth. He believed one of the objects Mr. Walker had in view in projecting the piers in the direction he did was, that the wave entering between the pier heads would expend itself on the beach within. He altogether objected to the theory that the best way of improving a harbour of this kind was by parallel piers. He knew that in the instance of Aberdeen, where there were partly parallel piers, the consequence was, that in easterly gales there was a heavy sea throughout the whole of the entrance channel, and a range even as far as the dock gates a mile distant. In the case of the Tyne, if the wave was cooped up between parallel piers, there would be a heavy range in Shields Harbour.

With regard to the question of the enclosure of the Jarrow Slake, he had always been of opinion that it was desirable to remove extensive indents from the banks of a tidal river, and to afford facilities, by dredging, for the passing of the tidal wave up the direct channel of the river. Therefore he quite conceived that by the construction of a parallel wall, and the enclosure of Jarrow Slake, together with the removal of the banks immediately above it, by which the tide flowed sooner into the upper part of the river, a great improvement had been effected. The main object required for the Tyne was the prolongation of the outgoing current. The greater the length of time, and the greater the velocity of the ebb tide as compared with the flood, the more the river, particularly at the entrance, would be improved. He was sanguine as to the good effects of the operations of Mr. Ure, after the piers had been sufficiently extended to permit the dredging away the shoals at the Narrows, and the formation of the bed to a regular inclined plane from Newcastle Bridge to the bar. The bar would probably be removed altogether. He would instance the case of a river somewhat similar in character and circumstances, the Dee, at Aberdeen. Formerly the tide used to flow over the whole of what was called the harbour, and it was only at half ebb that the proper river channel was defined. The greater portion of the area over which the tide formerly flowed was embanked with material chiefly dredged from the river channel. That was a case of facilitating the tidal flow upwards, and the bar, which formerly existed as a distinct ridge of sand, no longer existed. He thought the same result would be obtained in the Tyne by the well-judged operations which were now being carried on by Mr. Ure.

Mr. W. A. Brooks, in reply upon the discussion, said he had listened with great interest to the remarks which had been made, but he could not let them pass without some notice. It had been argued that the cause of the bar in the Tyne was the diminution of the velocity of the water of the river on entering the sea, and consequent deposit of the matter its waters previously held in suspension. That opinion was not satisfactory. No river on the east coast carried more silt and mud than the Humber, but that river had no bar; and many other rivers were in a similar state. The Thames carried down a great amount of silt, but there was no bar. As to the opinion that when the piers were carried out into deep water, the present depth of water on the bar, obtained by constant dredging, would be maintained in the channel, he thought the result would be different. If the dredging were given over, the bar would certainly shoal again directly. There was, however, already a satisfactory proof of that, by the vast quantity of material which had to be lifted by dredging every year, and the small result which was temporarily maintained in the shape of the depression of the bed or bar.

Mr. Stephenson had made some useful remarks, and his opinions coincided so much with nearly all that had been said in the Paper, that there was no occasion to comment upon them, except with regard to the utility of jetties as contrasted with parallel walls. It would seem that Mr. Stephenson had not had great success with jetties. But Mr. Brooks, by the employment of jetties alone, had been able to carry out works at small cost; and shoals had been removed, and important changes in the navigation had been effected, without the necessity of longitudinal walls, as well in tidal as in tideless streams. The large shoal and tortuous navigation above Bill Point were entirely removed, by the simple use of timber jetties, in one set of spring tides. That shoal had been a great impediment to the navigation; and on the occasion of the Author's closing up the ancient sailing channel, the pilot of the 'City of Hamburgh' steamer refused to take that vessel down. When Mr. Brooks volunteered to take charge of the vessel, the pilot changed his mind, and on the passage down the river said he had never gone down so pleasantly before.

He was satisfied that it was only by the vast dredging operations that had gone on for so many years that the bar was temporarily lowered, though the land-floods, which ran at a velocity of three or four knots an hour, must doubtless have assisted the dredging-machines. He was bound also to notice the shelter given by the north pier to the dredging on the bar. That dredging could not have been carried on if the north pier had not been executed to the extent it had; the bar being now completely under the shelter of the north pier. Every north wind would, previous to the construction of the pier to its present extent, have driven the vessel into the harbour for refuge. It was clear that effectual dredging operations could not be carried out without the protection of a pier. Mr. Redman, who was well acquainted with Mr. Walker's views as regarded the Tyne piers, did not appear to concur in the opinion of Mr. Walker, that the bar was caused by the conflict of the current from west to east with that of the current of the ebb at sea running from south to north, to which Mr. Walker had attributed the formation of the bar, but believed that it resulted from the conflict of the ebb from the river with the current at sea from north to south. That was precisely his own opinion, but with the reservation that the conflict occurred at the early part of the flood tide. With regard to the Mersey, although possessing a tidal range within the harbour of 30 feet, and several feet more range at sea, yet it only had a depth of 12 feet at low water on its bar.

Mr. C. B. Lane stated, through the Secretary, on the authority of Mr. B. B. Stoney, that the deepening of the bar of the Liffey was entirely due to the scour, and not to dredging. Dredging machines could not work on it. The Great North Wall, which projected 9,050 feet from the Clontarf shore, was commenced in 1820. The result had been the deepening of the bar from 6½ feet to about 14 feet at low water. The Great South Wall alone was inoperative; but as soon as the water was prevented from spreading over the North Bull by the Great North Wall, and directed in a definite channel, by the funnel form of the two walls in conjunction, the deepening of the bar commenced. This was the most successful instance on record of the deepening of an outer bar; and, Mr. Stoney had no doubt, greatly influenced the late Mr. Walker in his designs for the works at the mouth of the Tyne.

April 9, 1867.

JOHN FOWLER, President,

in the Chair.

The discussion upon the Paper, No. 1,152, "Memoir on the River Tyne," was continued throughout the evening, to the exclusion of any other subject.

April 16, 1867.

THOMAS HAWKSLEY, Vice-President,
in the Chair.

No. 1,172.—"The Suez Canal."[1] By Colonel Sir William Thomas Denison, K.C.B., R.E., Assoc. Inst. C.E.

The varying reports of the character of this work made the Author desirous of inspecting it on his way home from India. In order to do this, as thoroughly as time would permit, letters of introduction to the French Engineers were obtained from the authorities of the Company, and to Her Britannic Majesty's Consul-General from the Secretary of State. Consequently, on arriving at Suez, it was found that every facility would be afforded for inspecting the works of the Canal, and that means would be provided, by the different authorities, for securing conveyance to Port Saïd, and from thence to Cairo, after the inspection of the works had been completed.

Although furnished with copies of the correspondence between the late Mr. R. Stephenson and M. de Lesseps, as well as of Mr. Hawkshaw's Report, it was thought that it would be wiser to inspect the Canal, and to ascertain the views of the French Engineers employed upon it, and then to form, as an Engineer, an independent opinion, so far as such a mere hasty inspection would allow, before attempting to deal with questions which have assumed a mixed character—partly political, partly economical, and partly professional. Accordingly, while the aspect of the work was fresh in the Author's memory, such remarks as occurred to him were committed to writing, and the following notes comprise the substance of the opinions formed while inspecting the works. Allusion will afterwards be made to the report and correspondence before mentioned.

The scheme of the Suez Canal may be said to comprise two distinct undertakings. The first, and principal, is the construction and maintenance of a broad and deep salt-water channel on one level between Port Saïd on the Mediterranean, and Suez on the Red Sea. (Plate 13.)[2] The second, preliminary in point of time, and indeed essential to the construction, as well as to the beneficial use of the Canal, is the maintenance of a supply of fresh water sufficient for the wants of the population congregated along the line of canal, and specially at its two extremities.

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[1] The discussion upon this Paper occupied portions of two evenings, but an abstract of the whole is given consecutively.

[2] The plan and sections of the Suez Canal (Plates 13 and 14) have been compiled from "Compagnie Universelle du Canal Maritime de Suez. Carte générale de l'Isthme, etc., 1866," and the plans of Port Saïd and of the Port of Suez from "Percement de l'Isthme de Suez. Actes constitutifs de la Compagnie Universelle du Canal Maritime de Suez avec cartes et plans. Documents publiés par M. F. de Lesseps." 6e Série. 8vo. 1866.

The arrangements for the second of these undertakings have been completed. A canal, commencing at a place called Zagazig, to which water is brought from the Nile by one of the many branches from the main stream, has been carried to Suez, passing within about a mile or two of Ismaïlia, the central point of the main Canal, and the head-quarters of the establishment of the Company. It is navigable for the whole distance, the fall from Zagazig to Suez being overcome by locks. From the point where the Canal turns southward to Suez, a branch is carried first to Ismaïlia, where it provides a supply for the inhabitants, and for some hydraulic machinery, by which water is forced into a double line of 9-inch pipes, through which fresh water is carried along the side of the Canal, supplying the various establishments along the line of about 50 miles in length, and a population at Port Saïd already numbering upwards of 10,000; and secondly, for a distance of about a mile to the east of Ismaïlia, within which distance it is made to drop by two detached locks to the level of the Mediterranean. During the dry season the supply of fresh water is hardly sufficient to keep the navigation of the canal open; but it is proposed to carry a supplementary branch to Zagazig, from a point on the Nile above the "barrage" at Cairo. When this is completed, there will be an ample supply of fresh water at all times and for all purposes. At the Suez extremity the fresh-water canal terminates in a lock, by which vessels drop into the creek which brings goods and passengers from the anchorage to the town.

In the immediate vicinity of the anchorage a dry dock, capable of taking in the largest steamer, is nearly completed. The caisson to close the entrance was ready, or nearly so, when the Author visited the work, and the Engineers were employed in fixing the engine and pumps for emptying the dock. In front of this dock an "avant port," or basin, is in course of being dredged to a depth sufficient to receive large vessels. These works, however, are altogether distinct from the Canal, and are being carried out by a separate agency.

The salt-water Canal will commence to the south-east of this "avant port," the ground being dredged out to the necessary depth, and to a width sufficient to give ample space for the exit and entrance of vessels. From the character of the excavation for the dock, it is believed that the lower portion of the excavation for a depth of, say, 6 or 8 feet, will be hard enough to stand at a high angle. The Canal will sweep away in a curved line towards the north, passing to the eastward of the fresh-water canal through the lowest portion of the land, which is, in point of fact, very little above the level of the Red Sea.

Nothing, however, has as yet been done towards the commencement of the salt-water Canal, at the Suez end of the line, for the first 10 or 12 miles. At this distance from the sea, the line crosses a sort of spur from some hills to the westward, at a place called Chalouf, and there about three hundred men were at work upon a cutting, consisting partly of a bed of hard conglomerate, 8 or 10 feet in thickness, below which were strata of sand and clay. The surface of the soil at this cutting was said to be about 12 feet above the salt-water level, so that the total depth of excavation will be 38 or 40 feet. The slope of the side was 2 to 1, and there was a "bench" about 12 feet in width, 3 feet above the surface of the water; while another bench, 12 feet below the water, and 9 feet wide, formed a sort of base for the stone work with which it was proposed to face the upper part of the slopes. The stone was procured in part from the excavation itself, and in part from quarries on the west shore of the Red Sea a few miles south of Suez. The work here was carried on with a good deal of method. Inclined planes were cut in each bank, up which the wagons filled with spoil were hauled by steam engines, and then drawn to spoil banks at convenient distances. Pumps discharged the drainage water into a portion of an old salt-water canal, said to have been excavated by one of the Pharaohs, and the work was being pressed forward at a fair rate. Three hundred men, however, could not make much impression upon a work of such magnitude; the section of which is equivalent to about 350 cubic yards per lineal foot, the whole, it must be remembered, having to be lifted to the top of the bank.

Soon after leaving this cutting the Canal has to pass through the Bitter Lakes, the surface of the water, or soil, in which is nearly on a level with the Red Sea. Here the cutting will not exceed the ordinary section of the Canal; nothing, however, has been done to the work as yet.

The fresh-water canal passes at too great a distance from these lakes, and indeed from the rest of the line as far as Ismaïlia, to allow of the Author personally examining the line, or ascertaining what work was done, or doing. He was told that some dredging machines were at work, water sufficient to float them being admitted from the fresh-water canal.

The distance between Suez and Ismaïlia is about 50 miles, and it took twenty hours to get to the latter place, the average speed being 2½ miles per hour, but exclusive of stoppages about 3 miles per hour.

A little east of Ismaïlia the branch from the fresh-water canal came to an end, and the boat dropped, as before stated, by two single detached locks to the level of the Mediterranean, into a salt-water canal about the same size as the other. For some distance this was only a branch, but at about 2 miles from Ismaïlia the branch enters the line of the main Canal, which turns sharp to the left or northwards, and passes through a heavy sandbank from 40 feet to 60 feet above the level of the surface water in the Canal. The line did not seem to be well laid out here; it wound about, apparently to avoid cutting, but the result of this would be to render the passage of long vessels difficult. It was said that this portion of the work would be rectified.

The works here were in active progress; the Contractor was cutting down the face of the excavation, loading the spoil into wagons, trains of which were constantly in motion, being drawn by locomotives obliquely up the slope of the hill, and thence to spoil banks at some distance on the west side of the Canal. Some dredging machines were also at work deepening the channel; the soil raised being discharged into wagons and hauled up the bank. The amount of work to be done here was very great, as the sandbank or ridge extended about 5 miles.

To the north of the sandbank the Canal entered the beds of some lakes—the soil of which cannot be more than a few inches above the Mediterranean. Here, in places, the soil had evidently a disposition to slide into the Canal; as the trifling load of an embankment, sufficiently high to cover the fresh-water pipes, had forced the bank out, in spite of some piles and sheeting, which had been driven and fixed to support it. This sort of work extended the greater part of the remainder of the distance to Port Saïd, about 10 miles from which the Canal entered Lake Menzaleh, a shallow sheet of salt water, through which the line was marked, by slight embankments, distant apart the full width of the Canal. At several points between the deep-sand cutting and the shore of Lake Menzaleh, the Canal was opened to its full width. Dredging machines were at work both widening and deepening it, and some amount of activity was shown.

The dredging machines appeared to be well put together. They were worked by powerful engines, and a variety of expedients had been devised, for the purpose of adapting them to the work they had to perform, in lifting the spoil from a great depth, and discharging it at a point above the engine. Still, however, the impression conveyed, after watching carefully the working of these machines, was that the work must be expensive, and that any estimate of cost, deduced from the work done in shallow water, would be most fallacious, if applied, without very large allowances, to determine the cost of excavating the lower portions of the Canal, say the last 10 feet from the bottom.

The main work in progress at Port Saïd (Plate 14) was the jetty, or breakwater, which is to protect the port against the action of the north-westerly, or prevailing winds. This jetty is formed of blocks of concrete, rectangular in form, and weighing 20 tons. Experience has shown, that a block of this size (10 cubic mètres) and of this weight, is sufficiently massive to withstand the action of the heaviest sea. The concrete is composed of sea sand, dredged from the harbour, and of good hydraulic lime procured from Marseilles, the proportion of lime to sand being about 350 lbs. of the former to a cubic mètre of the latter, or about one to thirteen. Sea water is used to mix the ingredients, which are well worked and amalgamated in mills, ten of which are driven by one steam-engine. The mixture is poured into cases, or frames, of wood, where it is allowed to remain four days; the cases are then removed, and the blocks are subjected to the influence of an Egyptian sun for two months, by which time they are solid enough to withstand the action of the sea. Each mill can turn out three blocks per day.

The Contractor has engaged to furnish the blocks, and to deposit them at appointed spots, for 400 francs for each block. This sum is to cover the interest of capital upon the whole cost of the plant, the cost of dredging the sand, importing the hydraulic lime, mixing the material, and interest upon all this outlay for two months. The cost of conveying the blocks to the breakwater, and of depositing them, is paid for at the rate of about 1½ franc per cubic foot.

The mass will of course stand at a much steeper slope than it would were the blocks of smaller dimensions, and there will be a large space between the blocks, which will eventually be filled with sand drifted in by the sea. Still, however, it will not be safe to calculate the average section at less that 1,500 cubic feet, or the cost at less than 2,000 francs, per lineal foot.

At present a length of about 300 yards of the western arm is above water; and although the action of the sea on it is heavy, the material appears to stand well. Sand is accumulating rapidly along the western face; and it may be expected that a portion of the sand, which now chokes up the space marked out for the harbour, will be removed by the action of the easterly drift. A very large amount, however, will have to be dredged out. The cost of excavation by dredging machines is dependent upon a variety of contingencies. The French Engineer admitted that the cost of dredging was heavier than that of excavating by manual labour. The latter, however, cannot be got; while dredging machines and steam power are procurable to any extent which can be paid for.

It is true that, in theory, the whole expense of providing and maintaining the 'plant' is thrown upon the Contractor; but it may be surmised that advances must be made by the Company, for there were at Port Saïd forty large dredging machines, either under construction or repair, while sixty were said to be at work at different parts of the Canal. The cost of these cannot have been less than a quarter of a million sterling, and the establishments at Port Saïd, which had the appearance of a large dockyard, were maintained in great measure at the expense of, or at all events, principally for the use of, the Contractors. The cost of these must be very heavy, and fall upon the Company either immediately or eventually.

The result of this inspection of the works of the Canal is an opinion, that it will not be possible to maintain the section adopted by the French Engineers; that the slope of the bank, or sides, is far too steep to maintain itself against the action of the water, even when faced with stone, unless indeed the work can be done with better materials, and at a much heavier cost, than estimated; and that the estimate for maintenance, that is, principally, for removing drift sand and deposit, will be largely exceeded.

The effects produced upon the banks by the small vessel in which the Author was towed along the Canal, at a maximum rate of 3 miles per hour, were very marked. Not only were the banks cut away, but the *débris* assumed a slope differing, of course, according to the character of the soil, but in every case approximating nearer to that of an ordinary beach, than to that of the section proposed. As, therefore, most of the excavations will have to be taken out by dredging, in which case it will not be possible to protect the irregular slope left by the machine with any hard material except at an enormous expense, the Author cannot but think, that it will be found necessary to augment, largely, the surface width, and to give to the sides a slope of nearer 6 to 1 than 2 to 1. By thus widening the surface of the Canal, the action of the water raised by the passage of vessels will not be so injurious to the banks, as the wave would not be so high,—the section of the vessel bearing a much less ratio to that of the Canal than according to the present section. There are other advantages which would result from the increased width of the Canal to which it is unnecessary to allude.

The correspondence between M. de Lesseps and the late Mr. R. Stephenson, in which the late Mr. Rendel, Mr. M'Clean, and Mr. Manby appear to have taken part, was principally as to the relative merits of a canal according to the proposed construction, and of one at a higher level, drawing the water required for lockage and waste from the Nile, and dropping by locks into the sea at each end. The whole gist of the arguments lies in the statement made by the English Engineers, that an open canal upon a level between the two seas was "an impossibility." To this view the Author cannot yield assent.

The second document referred to is the Report of Mr. Hawkshaw, dated February, 1863. While adopting generally the statements and opinions therein expressed, especially those dealing with the engineering objections to the present scheme, the Author is inclined to think that the difficulties to be encountered, and the cost of maintaining the work, have been under-estimated.

The opinion the Author has arrived at, based upon what he saw and heard during his visit to the Canal, and upon a fair consideration of the correspondence and report before alluded to, may be stated as follows:—

1st. That (subject, of course, to the condition that the relative levels of the Red Sea and the Mediterranean are as stated by the French authorities) there will be no extraordinary difficulty in carrying an open salt-water channel from the Mediterranean to the Red Sea of the depth proposed, namely 8 mètres.

2nd. That no special difficulty in maintaining this channel need be anticipated.

3rd. That it will be necessary to modify the section proposed by the French Engineers, making the side slopes much more gradual.

4th. That the cost of maintaining the above-mentioned depth of water will be found at first to be largely in excess of the amount estimated. Eventually, it is by no means impossible that means may be found to fix, or check the drift of sand, or to shut it out from the Canal. But for some years it must be expected, that the ordinary action of the atmosphere, which has filled up former excavations made in this dry desert, will have the same effect in the new Canal.

Looking at the work as an Engineer, there does not appear to be any difficulty which a skilful application of capital may not overcome.

Sir W. Denison said, he had nothing to add to the Paper, further than to state that he had brought it forward in the hope of eliciting such remarks, both on the principles and details of the Canal, as might either justify the opinions he had formed, or lead him to acknowledge he was in error, if he felt he was so.

Mr. Abernethy thought it necessary, before giving an account of his visit to the Canal, and of the present state of the works, to point out a few particulars which had not been touched upon in the Paper, and might be interesting to the Meeting.

At a remote period—stated to be six hundred years before the Christian era—a fresh-water canal, distinct traces of which were still to be found, was completed by Darius Hystaspes. It commenced near Suez, and could be traced as far as the Bitter Lakes, and from them to Lake Timsah, near which it was joined by a canal from the Pelusiac branch of the Nile. From Lake Timsah there were also traces of a canal leading towards Lake Ballah, and no doubt it communicated by Lake Menzaleh with the Mediterranean. From all appearances the Bitter Lakes at one time formed part of the Red Sea, while, on the other side, the Mediterranean probably extended to the high ground near El-Ferdane, so that the distance between the two seas was at one period about 90 kilomètres, as against 160 kilomètres at present.

As to the state of the works last February (Plate 13), commencing at Suez, a dredging machine was at work on the site of the proposed entrance basin; and, for a length of 8 kilomètres or 9 kilomètres towards the Bitter Lakes, the Canal was excavated to its full width for a depth of 2 mètres, the material being hard gravel on the surface, with beds of sand and clay beneath. The fresh-water canal was finished, and on a short branch from it at Chalouf, dredging machines were about to be used in excavating the marine canal. At Serapéum, by means of another branch, canal water had been admitted into a depression of the desert, and an artificial lake had thus been formed in which several dredging machines were at work forming the bed of the marine canal. From Ismaïlia to Port Saïd, a distance of 40 miles, the Canal was excavated, for a portion of its width, to a depth of 8 feet or 9 feet, and the water of the Mediterranean filled the bed of Lake Timsah. Here, and throughout the length of the Canal to Port Saïd, an extensive dredging plant was at work.

A good deal had been said about the alteration of the slope of the Canal, to which attention appeared to have been directed by the French Engineers after Mr. Hawkshaw had made his report. At the end of last year it was finally determined to widen out the surface of the Canal from 188 feet to 328 feet, the bottom being 72 feet wide, the slope of the bank $2\frac{1}{2}$ to 1; then a 'berm' of 50 feet, and slopes of 3 to 1 and 5 to 1, so as to form a flat beach on which the wave from the passing vessels would expend itself without injury to the banks (Plate 14). The part of the Canal through Lake Menzaleh was through alluvial deposit similar to that of the Valley of the Nile. At Port Saïd the western breakwater had been extended 1,550 mètres by means of béton or mortar blocks, formed in the manner described in the Paper. The effect so far had been to advance the foreshore about 800 feet on its western side. The channel varied in depth from 16 feet to 13 feet. From forty to fifty vessels were lying in the port, including a steamer of about 1,000 tons.

The impression he had formed, with regard to the Canal generally, coincided with what Mr. Hawkshaw had stated in his report, that in its execution there were no engineering difficulties; but that it was simply a question of time and money. He did not agree with the Author of the Paper that the maintenance would be a very serious matter. There were two or three points, however, which deserved attention:—The first was as to the effect at Port Saïd of the jetties in arresting the sand brought from the westward, how far the foreshore would eventually be advanced, and whether it would lead to the formation of a bar or sand-bank at the entrance. The second point was as to the effect which would be produced by the admission of a large body of salt water into the Bitter Lakes, which were at least 25 miles long and 5 miles wide—shallow—without any current—and exposed to the action of the fierce sun of Egypt. The third point was the maintenance of the banks of the Canal against the wave caused by the passage of vessels.

With regard to the dredging operations in Lake Menzaleh, there were, at the time of his visit, about sixteen machines, capable of lifting from 1,000 cubic mètres to 1,500 cubic mètres per day, working generally in the centre of the cutting. The mode of depositing the material dredged out was by means of long spouts supported by a supplemental barge between the machine and bank, some of the spouts being 200 feet in length. Pumps were provided to throw water into the spouts, so as to liquify the material sufficiently to cause it to run to the canal bank. He observed that the water drained off the material very rapidly, and that a solid bank was formed, more so than by the ordinary mode of depositing spoil. He did not agree with the Author of the Paper, that manual labour in that country was cheaper than dredging by machinery. His own impression was, that in such a country as Egypt dredging was the cheapest form of labour that could be employed in a work like this. He believed that dredging was forced upon the Engineers, in consequence of the cessation of forced manual labour, and he thought it would have been well if that cessation had occurred at an earlier period. The heaviest and most expensive portion of the dredging yet remained to be done; as in most parts only the 'top lift' had been removed to the extent of about one-third of the whole amount of excavation.

The concrete blocks of the breakwater were deposited in a given line without order, so as to form an artificial reef.

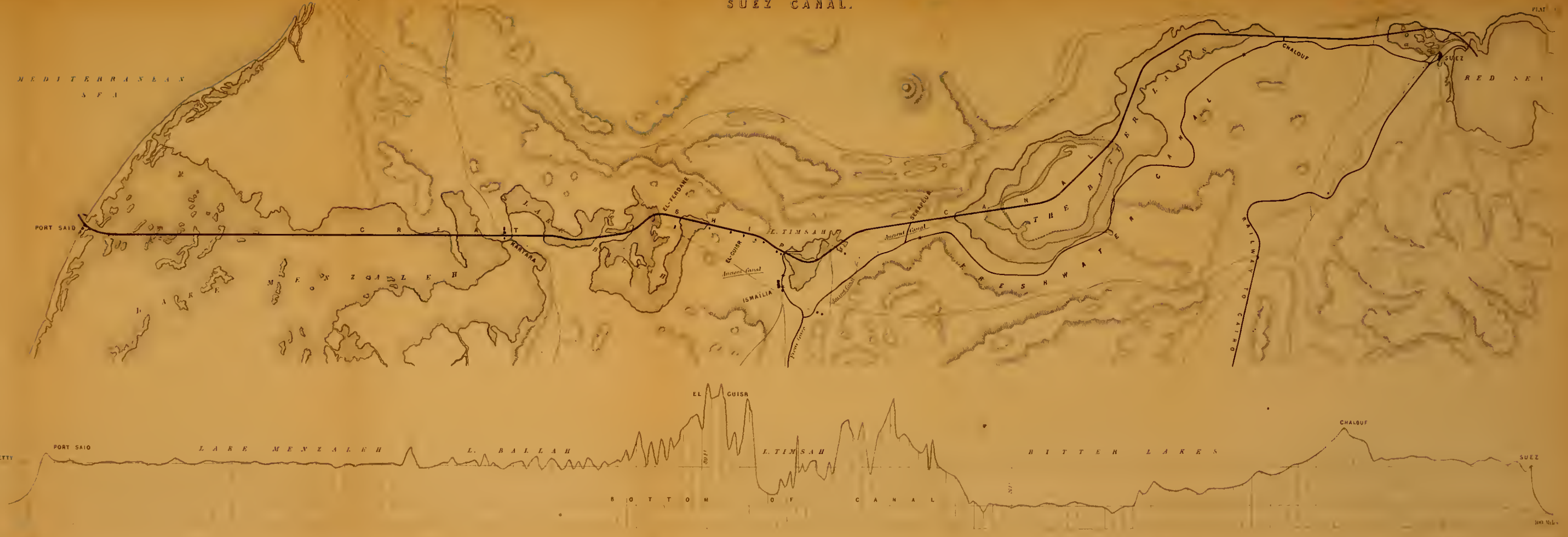
Sir W. Denison remarked, that the officials were chary of giving information on the subject of cost; but the Engineer told him the time for completing the works would be three years from the middle of 1866. He confessed he was disposed to think it would be six years. With regard to the embankments, there were none, except just through those lakes which were on a level with the sea, and they only required to be of a height sufficient to carry the water pipes. There were no embankments of any great height.

Mr. Hawkshaw said, such information as he might be able to afford applied to a date anterior to that of Mr. Abernethy's visit to these works. All he could speak to was as to the character of the works which required to be executed. He could only repeat, what he had already said in his report, after a careful investigation, that there was no difficulty in the works. Though large, they were of the plainest kind; for he did not know that there was a single work of art from one end of the Canal to the other, with the exception of the sea-jetties. There was no bridge, no lock, no sluice, nothing, in fact, to execute but the sea-jetties and a large excavation, which had to be done about half in the ordinary way of dry excavation, and the other half by dredging. The cost would necessarily be affected, and to a most serious extent, by the length of time which might be spent in completing the Canal. In 1862, when he was in Egypt, the French Engineers considered that, with thirty thousand men, they could complete the Canal in three years. He was at that time informed by the Viceroy, that it would be impossible for him to furnish so many, and that he could not find more than twenty thousand men. That of itself would have extended the time for the completion of the Canal to about five years; but he had reason to know that so far from even twenty thousand men having been supplied, the number had been comparatively few, and consequently much time had been lost; and in a work like this, where interest was paid out of capital, if the work lasted long enough, it might be made to cost any sum. The want of men might to some extent be supplemented by the introduction of dredging machines, which he was informed was the case. It would be impossible to execute the dry excavation by dredging; but where the work admitted, the excavation might be accelerated by multiplying the number of dredging machines. He thought the question of maintenance would not prove a serious one, always assuming the Canal to be finished in a workmanlike manner, and he had no reason to believe the French Engineers would do otherwise. The opinion he gave in his report on the cost of maintenance was based upon the assumption, that the slopes would be made flat enough, and that the 'berm,' which was to be widened, but which could not be very wide, would be protected by stone, and the whole of the works completed in a proper manner. If that were done, he believed the sum put down for the maintenance of the Canal would be sufficient. The cross section which Mr. Abernethy had referred to as being now carried out did not quite meet with his approval. The slope, he thought, having regard to the kind of material excavated, should be 3 to 1. It appeared to be 2½ to 1. He also thought it would be necessary to cover the 'berm' with stone. He noticed that the bottom width had been made narrower between the Red Sea and the Bitter Lakes, which, if adopted, would be a smaller section than the one alluded to in his report.

There were one or two important questions for consideration with regard to the drift of sand. Between the Red Sea and the Bitter Lakes there would be no drifting of sand of any moment, as the sand was generally compact, and much mixed with gravel. Nor would there be any between the Bitter Lakes and Lake Timsah, except for 8 or 9 miles near Serapéum; but between Lakes Timsah and Ballah, principally at El-Guisr, there would be drifting. For a distance of about 10 miles in Lake Menzaleh there could be no drift sand, so that there were only two places, together amounting to about 19 miles, where it would occur. He had had to do with drifting sands in Russia and other places, and by proper precaution it could to a certain extent be checked; and he believed the drifting in these two places would be comparatively small in quantity. He did not see why the maintenance of those portions of the Canal should cost more than was the case in canals in Holland, in this country, and elsewhere. The only other place where apprehensions of difficulty had been entertained was at the entrance of the Mediterranean. It was said the harbour would silt up. He had no doubt in course of time it would, to some extent, as all harbours did more or less; but it would require a long period of time. The breakwater would be 3,000 mètres in length at least, and the growth of that portion of the coast, though it might be what geologists would call rapid, in an engineering point of view would be exceedingly slow, and need not occasion any anxiety; the harbour might be maintained with occasional dredging. If it should become necessary at a remote period of time, the piers could be lengthened. There was another point: it was said that the Bitter Lakes would in course of time fill with salt. These lakes were dry at the time he visited them. He passed along the bottom of the whole of them. How long they had taken to dry up he could not say; but the result was the residuum of salt which

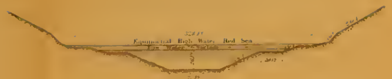
SUEZ CANAL.

PLATE

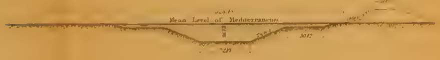


SUEZ CANAL.

PLATE 14



CROSS SECTION Nº 1



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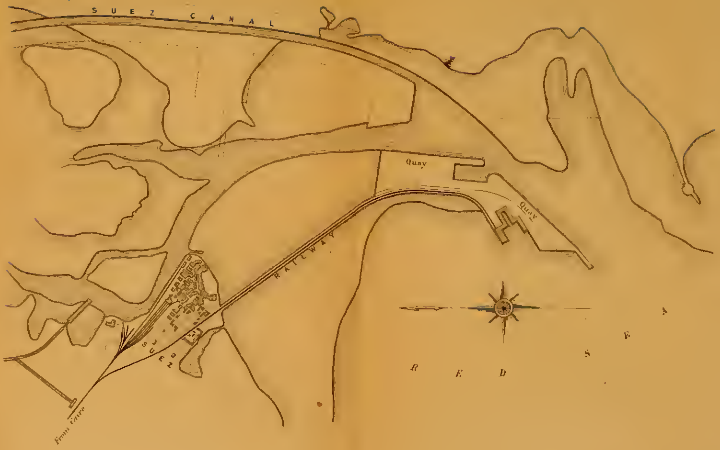
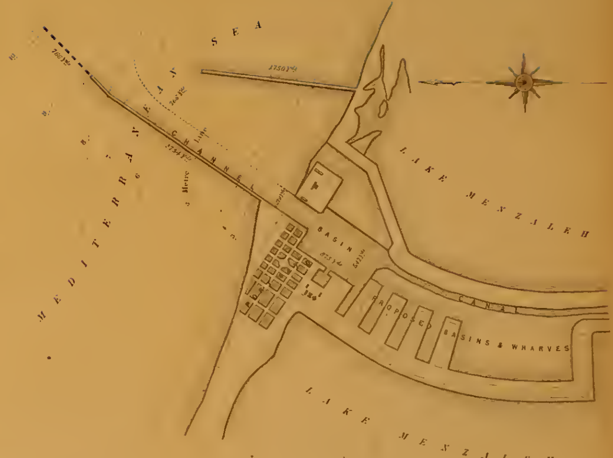


PLAN OF PORT OF SUEZ.



PLAN OF PORT SAÏD.

remained, and which represented the whole quantity that had been derived from evaporation for possibly hundreds of years, was just 6 inches thick. He had dealt with the question of salt from evaporation thus: he had assumed an evaporation from the Bitter Lakes of about 10 feet of water per annum, and from the quantity of salt contained in sea-water, he came to the conclusion that if the whole of those 10 feet of water were evaporated and the salt precipitated, it would produce a stratum about 2½ inches thick. But before sea-water would deposit salt at all, it would require to be reduced by evaporation to one-tenth of its bulk; but this could not take place in the Lakes, where water daily flowed into them at each end. It could only be effected by shutting out the water, which would flow into these lakes with great rapidity. There was another question which had been raised: it was said by some that the Canal would become a stagnant ditch, while others held that the current through the Canal would be such as to tear away the sides. He had investigated this question, and he estimated that between the Red Sea and the Bitter Lakes there might be a current of about 3 feet per second, and between the latter and the Mediterranean a current of about 6 inches per second. With such a current it would be necessary, unless the material proved to be hard, for the sides to be pitched between the Red Sea and the Bitter Lakes, and that was one of the items of expense mentioned in his report. In coming to a conclusion as to the cost of maintenance, he assumed that all those things necessary to complete the work would be carried out; otherwise his valuation would not apply. He had only to answer the question whether the construction of the Canal was practicable. He had nothing to do with the question whether, when made, it would be commercially valuable. That was a matter for those individuals who had embarked their money in it. He had not knowledge enough of the cost of work in a country like that to say precisely what the Canal would cost; but the general conclusion arrived at in his report of 1863 was—taking the estimates of the French Engineers, and adding to them what appeared to him essential to render the Canal available and durable—that the cost would amount to about £2,000,000 more than had been estimated, if the work were completed within the five years. What had been already paid for interest he did not know, though it must amount to a large annual sum.

He was connected as Engineer with a ship-canal in Holland, which, in capacity, was as large as the Suez Canal, but the works themselves were more varied, involving locks, sluices, pumping-machines, &c. It ran from Amsterdam to the North Sea, and entered the sea on a flat shore, and extended, for a large portion of its course, through shallow lakes of about the same depth as Lake Menzaleh; but he thought the dredging was managed in a cheaper way than in Egypt.

In Holland the plan of operation was this: the dredging machine lifted the material, which consisted of clay and silt, into a vertical cylinder attached to the machine at such a height as to receive the material from the buckets as they revolved. At the bottom of the cylinder was a Woodford's pump, which acted somewhat similar to a pug-mill. The pump, revolving, forced the material out of the bottom of the cylinder through a floating tube, made of wood in lengths of about 15 feet, each length being attached to its neighbour by a flexible junction of leather with a spiral wire inside, and the end of the tube was carried over the canal bank, and was managed with as much ease as the hose-pipe of a fire-engine. In that way the material was deposited where required in a much cheaper, and he thought in a better way than by the dredging machine used in Egypt. It was seldom that water had to be supplied to the material, inasmuch as the buckets generally came up with a quantity sufficient to keep it in a liquid state, so as to allow of its passing along and out of the end of the tube. Water could be admitted into the pump or cylinder, so that the necessary degree of fluidity could be given to the material to drive it along the tube.

Mr. Murray wished to know what was the cost of dredging on the system just described. In the Suez Canal the cost was stated to be 1 franc per cubic mètre.

Mr. Hawkshaw said that was the Contractor's, rather than the Engineer's business; but he should say the cost was not more than one-third of what was stated in the case of the Suez Canal. The rise of tide in the Red Sea was about 3 feet 6 inches on an average; but on extraordinary occasions, with peculiar winds and equinoctial tides, it reached 5 feet. In the Mediterranean, though it was regarded as a tideless sea, there was a tide at Port Saïd of between 8 inches and 9 inches, and on extraordinary occasions of 2 feet above the mean level of the sea. The relative levels of the Mediterranean Sea and of the Red Sea were the same. The current would not be created by the difference of level, but mainly by the rise and fall of the seas at each end, and partly by the evaporation which would take place from the surface of the Bitter Lakes.

Mr. Abernethy stated, in reply to questions, that he was not able to say what proportion of the expenditure up to the present time was due to the execution of the 20,000,000 cubic mètres of excavation completed, leaving 40,000,000 cubic mètres yet to be removed. The expenditure on the latter was estimated by the Engineers to be about £4,000,000 sterling.

Mr. Robert Rawlinson, C.B., said any observations he could offer upon a work which he had not seen would not be of much value; but from past experience and from a practical knowledge of what ground would do under similar conditions, he had come to the conclusion that the cost of maintenance on the Suez Canal, when completed, would be a very serious item. In railway cuttings in this country, slopes of 2 to 1 had remained apparently solid, when only subjected to the ordinary action of the atmosphere, and yet, after standing ten years or fifteen years, great masses of earth slipped and came down suddenly; and he thought that the cost of maintenance on this Canal through the sandy district of its course would be a most serious item, especially with slopes of only $2\frac{1}{2}$ to 1. Where the bottom of the Canal was sand, there would be a continual tendency of the bottom to rise, as well as of the sides to slip; that action went on in many river-courses. If a river-course had not upland water coming in to keep it open by scour it would soon silt up; and in all level alluvial valleys there was a tendency in the channels to alter their courses by the sides slipping in and the bottom silting up. He therefore considered this Canal, in the portion through the sand, would necessitate a considerable sum for maintenance. The Clyde and the Tyne had been vastly improved by dredging, but that operation had to be continued, and was kept up because the trade of the ports could afford to pay for it; but if that were not done, those rivers would revert again to their former state. With regard to the pitching of the sides of the Canal, Mr. Hawkshaw mentioned rubble; but one need not suggest to French Engineers the use of concrete, as they had sand in abundance, and a large proportion of the pitching or facing might be done in concrete much cheaper than in rubble. Some engineering works in Great Britain, recently completed, had not proved commercial successes. It had become fashionable to talk, when vast projects were under consideration, of discarding the word 'impossible:' it would, however, be more in accordance with common sense to use the commercial word 'profitable.'

Mr. Lange said, as the representative of the Suez Canal Company in this country, he had listened with deep interest and attention to Sir William Denison's Paper, and the subsequent discussion upon it. He did not come to the Meeting with the view of taking part in the present controversy: having, however, been called upon by the President to address the Institution, he would confine himself to a few observations with regard to the actual position of the works.

He would premise his observations by expressing some regret that Mr. Hawkshaw, whom he had the pleasure of accompanying during the official inspection of the Suez Canal works, for the former Viceroy of Egypt, did not suggest the more economical method employed in important canal works in Holland, assuming he was acquainted at the time with the plan now adopted on the Dutch Canal. The suggestion might have enabled some economy to be effected in the expense of the long shoots attached to the dredging machines, which had been found necessary to deposit the silt at great distances apart on the banks of the Egyptian Canal. The applicability of Mr. Hawkshaw's system, however, to the Suez Canal, would greatly depend upon the elevation that could be given to it. Certain it was that the sections of the Suez Canal were very different from those met with along the one projected in Holland, where no elevations existed such as were found between Port Saïd and Suez.

He could not agree with all that had been said by Mr. Abernethy; at the same time he admitted the difficulty he must have experienced in gathering detailed and reliable information during a somewhat hasty survey. Notwithstanding Mr. Abernethy's statement as to the number of dredging machines actually employed along the line of the Canal, was made in perfect good faith, he must beg to observe that the number was not so great as he supposed.

Although not strictly a professional Engineer, it was impossible, after being connected with the Suez Canal from its origin, which was now ten years ago, to remain unacquainted with the engineering bearings of the question. While he was familiar with the views and opinions of every Engineer of note who had written or spoken on the subject, he had at all times refrained from putting forward any engineering opinion of his own, but pinned his faith to the best evidence he could obtain.

Having stated this, he would confine his observations to the actual state of the works, and he thought it would be of interest to the Institution to know how much work had actually been done, and what still remained; also by what means those results had been attained, and at what expense.

The entire quantity of material to be excavated was 70,000,000 cubic mètres; but up to the present time 23,000,000 cubic mètres only had been raised. This might appear a small proportion, comparatively speaking, with what ought to have been done, considering that one half of the original capital (£8,000,000) had been expended to obtain this result; and, by parity of reasoning, the remaining £4,000,000 might be supposed insufficient to excavate 47,000,000 cubic mètres, and complete the works. He would endeavour to account for this apparent disproportion.

Previous to entering on a work of such magnitude as the Suez Canal, it was necessary to prepare dwellings, storehouses, forges, workshops, a lighthouse, and piers, in what was then the most desolate spot that could well be imagined, shut out as it was from all approach, except by the most tedious and expensive modes of transit. There were, besides natural obstacles, others of an extraneous character, which, being of a political nature, it would be out of place to mention at a meeting of Engineers; but the most serious delay was experienced, and much time was lost, in effecting the entire transformation from one system to another, imposed upon the Canal Company in consequence of the withdrawal of native labour, and the substitution of mechanical appliances and European skilled labour. Men had to be recruited from different parts of Europe, and it was very difficult at first to procure them, owing to their objection to labour in the heat of the climate of Egypt. All this was harassing in the extreme, and took time to accomplish. Then, again, the new system of larger dredging machines necessitated also the widening of the accessory canals for their conveyance; moreover, additional dwellings had to be provided for the Engineers, and from small encampments large towns had gradually sprung up along the banks of the Canal, suitable to accommodate the additional influx of European labourers.

During all these new preparations, comparatively little digging could be done; and hence arose the fact, that compared with the outlay of money, the disproportion was so great between the quantity actually excavated and what still remained to be done. These obstacles had been entirely overcome, and the Company was now in a most satisfactory position.

According to the last reports from Egypt, the quantity of material excavated in the month of February in the divisions of El-Guisr, Suez, and Port Saïd, amounted to 816,074 cubic mètres; in the division of Ismaïlia, 142,988 cubic mètres; together, 959,062 cubic mètres, being 24,000 cubic mètres in excess of what was raised in the previous month; and in the month of March it was expected that about 1,000,000 cubic mètres would be excavated.

Now, although this was a very fair return for one month, larger, he believed, than any hitherto achieved by similar means, in the annals of engineering history, it might be of interest to the Institution to know how many dredging machines had actually been employed to obtain it. There were forty dredging machines on the spot, but of these only thirty-one were in working order. Thus, with thirty-one dredging machines and seven thousand labourers, nearly 1,000,000 cubic mètres per month were excavated, and this quantity would go on increasing in proportion as the machines could be got in readiness. Many were now on their way to Egypt, and it was fully anticipated that no less than seventy dredging machines would be ranged along the line of the Canal before the ensuing autumn.

The members of the Institution would now be able to judge whether the Canal Company were justified in anticipating a return of 1,500,000 cubic mètres per month, which, if accomplished, would finish the work in less than thirty-three months. As he had already stated, there remained about 47,000,000 cubic mètres to excavate, and at the ratio mentioned, 49,500,000 cubic mètres could be excavated in thirty-three months, or 2,500,000 cubic mètres in excess of what now remained to be done. This was taking a twelve hours' day, without relays at night, which might be had recourse to if necessary.

Contracts had been entered into with well-known and responsible Contractors, which induced the Company to believe that the original capital would not be exceeded. At the same time it must not be overlooked, that in consideration of the withdrawal of native labour, the retrocession of large grants of land, and the abandonment of other privileges attached to the Act of Concession, the Suez Canal Company were to be indemnified by the Viceroy of Egypt to the amount of £3,800,000; and he thought that with that addition to the original capital, the Company were justified in the belief, that it would go far to meet any unforeseen contingencies, and warranted the expectation that this gigantic work, which at one time was regarded as a visionary scheme, might be brought to a successful termination in thirty months from the present time.

Mr. W. A. Brooks considered that the real difficulty of the case consisted in the sand that would be brought in from the vast area of the Nile Valley. Whatever might be the difficulty of maintaining the banks, it would be as nothing to that of keeping the Canal clear of sand deposits, whether brought in by northerly and north-westerly gales raising the level of the Mediterranean, or by similar effects produced by the tides of the Red Sea, aided by southerly gales gorging the cut at the other end.[1] If physical difficulties were apprehended in keeping open a communication at a time when it was supposed that there was a difference of level between the two seas of 30 feet, how much more would those difficulties be increased now that it was ascertained that the difference of level in spring tides was only about 5 feet 6 inches? The difference was so slight, that for only a short distance would the current be able to run towards the Bitter Lakes. He doubted whether it would be able to keep those lakes full by the system which had been adopted. He questioned whether any Engineers who had considered this subject, from the time of Napoleon's expedition into Egypt, when it was first suggested to make a ship-canal, down to the present, had been fully aware of the great physical difficulties to be encountered. The late Mr. Rendel, and the French Engineer, M. Talabot, recommended a ship-canal, to be supplied from the waters of the Nile, the communication between the Red Sea and Mediterranean Sea being partly through the channel of the Nile. There would be no difficulty in thus filling and maintaining a canal from above the barrage of the Nile, which was at some 20 mètres' elevation above the mean level of the Mediterranean. By that arrangement it would be possible to store the fresh water, and bring it into the Canal throughout its whole length, and thus supply the power of irrigating vast adjacent tracts of land, as well as of accommodating the navigation. There was reason to believe that in pre-historic times, a natural channel connected the Mediterranean Sea with the Red Sea, which could have been separated by a course of only a few miles, as appeared from the geological features of the country, and the proofs afforded by the borings which had been taken. He was surprised to hear that such a small extent of recent deposit had been produced by the western currents from the Nile, after hearing Mr. Abernethy's statement, that the jetty had been carried out to the extent of 1,500 mètres, or 1,625 yards. Mr. Brooks had found in the Library of the Institution a work on Littoral Currents by Commander Alexander Cialdi,[1] which gave an account of the progress of the Port Saïd jetty. In this it was stated, that the Engineer Kramer reported in July, 1865, that there existed only a small portion of the western dyke, of a length of 400 mètres; and that along the whole exterior of this section of the western pier there had formed a bank of sand which extended at the back of the pier for its entire length, while its base, resting on the shore, stretched to the westward for a length of 1,700 mètres. Commander Cialdi also stated that a previous writer on this subject, M. Guglielmotti, entered in his journal, under the dates of 15th to 19th of February, 1864, that, at that time the western jetty consisted only of open piling, extending into the sea for a length of 364·50 mètres; and that about 20 tons of rubble had been deposited at the end of the work for its protection; that the wind was almost always from the west, and that the water which passed before him from the left to the right was always troubled, and of a yellow colour. On this Commander Cialdi observed, that only seventeen months had elapsed during which this great accumulation of sand had taken place. He also stated that an isolated deposit of blocks of béton had been formed, at a distance of 1,500 mètres from the shore, of 60 mètres in length, but on which, at present, there was no accumulation of sand; but that it was certain, when this portion was connected with the part constructed from the shore, the accumulation of sand would take place. Mr. Brooks wished to be informed how much of this gap of about 1,076 mètres in length had been filled up since the record of the state of the pier by the Engineer Kramer in July 1865? Knowing the length of period which the construction of such works necessarily occupied, it appeared to him extraordinary that even a fourth of that gap could have been since completed to the full height of the intended pier.

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[1] A plain of 3,600 square miles bore testimony to the extent of the deposit brought through those influences; viz., the currents of the two seas added to the littoral current from west to east, charged with the alluvium of the Nile.—W. A. B.

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[1] *Vide* 'Les Ports Canaux.'—"Article extrait de l'ouvrage sur le Mouvement des Ondes," &c. Par le Commandeur Alexandre Cialdi. Rome, 1866.

Mr. Abernethy remarked, that a length of 1,500 mètres was executed in blocks of béton, but he could not give particulars of the number of tons deposited.

Mr. Brooks could not conceive how it was, that the causes which produced a silting up to the full extent of the 400 mètres should have ceased altogether on the farther extension of the pier works to 1,500 mètres. He would ask what changes there had been in the bed of the sea, between the old high-water line and a distance of 1,500 metres from the shore, and whether these original soundings could have remained the same, notwithstanding that enormous protrusion of the pier? Looking at the admitted result of deposit of sand to the full extent of the first formed length of 400 mètres, he should have thought a similar accretion must have ensued from the extension to the greater distance of 1,500 mètres. With regard to the borings taken in the line of the Suez Canal, those made in the neighbourhood of the cutting through Lake Menzaleh showed a depth of 44 feet 10 inches of Nile mud and sand below the mean level of the Mediterranean Sea; or of material, in fact, as bad for the foundation of the piers and wharves of Port Saïd as it would be found to be costly in the formation of the Canal, on account of the long slopes which the banks would require where the cutting was through such soft material. Mr. Hawkshaw reported on the Suez Canal,[1] that—

> "The tidal action at Suez, consisting of an alternate elevation and depression of the water with reference to the mean level of the sea, would lead to an oscillating current into and out of the canal, which would alternately raise and lower its level as far as the influence of the tide extended, which for all practical purposes may be assumed to be as far as the Bitter Lakes only. This would produce not only an inward current, but also an alternating outward current of nearly the same velocity as the former. The same action from the like cause, though in a much smaller degree, will obtain at the Mediterranean end of the canal. It is probable that the action of these alternating currents will be to carry out as much sedimentary matter as they bring in."

He entirely differed from Mr. Hawkshaw as to the result which would take place as to the return to sea of the sand brought into the Canal by the flood tide. Assuming that the Bitter Lakes would only attain the level of half tides in the Red Sea, in the one case there would be at the mouth of the Canal the rise of spring tides in the Red Sea, and at the other end, or at the Bitter Lakes, there would be no lift of tide at all. The result would be to bring sand into the channel of the Canal, with a diminution of power to carry it back again; and that would cause the Suez Canal to be for nearly the whole of its course, that which the late Mr. Robert Stephenson had very aptly called it, "a stagnant ditch." There appeared to be a small difference in the mean levels of the two seas, as deduced from the tidal observations which had been made; the mean level of the Mediterranean being 2 feet 3 inches lower than that of the Red Sea, and there was nothing to authorise the supposition that there would be a return current into the Mediterranean. In 1860 he wrote a Paper on the subject of the Suez ship-canal,[1] and the whole of the details of the levels of the two seas, as affected by the tides and wind, were given in the Appendix to that Paper. With regard to the amount of the deposit of marine salt in the Bitter Lakes, it had been stated that it did not exceed a depth of 6 inches in thickness; whereas the borings showed that below the bed of the smaller Bitter Lake there was found a bed of 6 feet of marine salt, and in the greater Lake 13 feet, neither of those borings having, however, penetrated through the bed of marine salt.

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[1] *Vide* "Suez Canal.—Report to the Egyptian Government," p. 19. London, 1863.

He wished to direct attention to the distinction between the tidal phenomena in spacious channels, like the Thames and the Humber, and those observed in a restricted channel, like that which was proposed to be made for the Suez Canal. Take, for example, the artificial cut or present channel of the River Yare (Yarmouth) between the sea and Lake Breydon. There was a length of channel of only about 3 miles between the sea and the lake, yet for all that the level of Lake Breydon at its north end was 3 feet below the level of spring tide at sea; showing that, although Lake Breydon was only about 2½ miles long, the tides were paralyzed by passing through its waters, and by having to encounter the restricted channel between it and the sea. That the Yare navigation afforded a good illustration of the effects of a restricted channel, as compared with the proposed cut from Suez to the Bitter Lakes, was proved by the tidal range in Yarmouth Roads being about the same as at Suez. There was another instance in the Nene of the effect of a restricted channel preventing the stream maintaining the same high-water level as at sea; but at the mouth of the Nene there was a rise of tide of 23 feet, and yet there was a depression of the high-water surface, owing to the restricted channel, of as much as 13 feet 6 inches. The inference from this was, that even had there really existed a difference of level between the two seas of 30 feet, the restrictions offered by a narrow artificial channel of 90 miles in length would have been so great, that, as in the Nene, the tidal wave would have been throttled. Final success in maintaining a navigation could only be obtained by increased dimensions of the cut connecting the two seas. In the Witham, again, where the rise of tide was 23 feet, there was also an unusual depression; and therefore, he said, there must be an absence of current, except within a few miles of the mouth of the cut, to return to the sea any sand brought into the Suez Canal by the flood tide. Incessant dredging upon a great scale would therefore be required to keep the channel open. With regard to the section of the cutting for the ship-canal, the bottom showed a breadth of only 70 feet. The vessels that would navigate the Canal might be taken at 40 feet beam, and paddle-wheel steamers would have 10 feet more on each side, thus making their entire breadth 60 feet. It would thus be quite impossible for two vessels to pass each other, as one must go upon the bank, and would have to discharge part of its cargo to get off again. The presence of the dredging machines would also cause an obstruction. With regard to the slopes, he did not for a moment think that the cutting in mud and sand—Nile mud—would stand at even 2½ to 1. It was more probable they would require to be 4 or 6 to 1. He thought that the system of dredging, as carried out, was objectionable; in his opinion the spoil banks should be formed at a considerable distance, instead of being so near the margin of the cut, as was shown by the section of the works. Indeed, he felt certain that the spoil would find its way again into the channel, and would have to be dredged afresh. He would refer to the published opinions of the late Mr. Robert Stephenson on the subject of the Suez Canal, in reply to a letter of M. de Negrelli, one of the International Committee of Engineers, who ventured to censure Mr. Stephenson for the opinions he had expressed on this matter. M. de Negrelli said in one part of his letter:[1]—

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[1] *Vide* "The Civil Engineer and Architect's Journal," vol. xxiii., p. 169, *et seq.* London, 1860.

> "I do not share the opinion of my honourable friend in England, that the canal without a current must become a ditch where the water will be perpetually stagnant."
>
> "The great basins in the interior of the isthmus will form a considerable surface of water, and will maintain, as all interior lakes, a constant motion. The difference between the tides in the two seas will communicate to the canal the agitation which they possess themselves. The canal can only be considered as the continuation of the two seas whose waters meet in the basins. If my honourable friend will look from the windows of the building in which he has developed such singular hydraulic knowledge, he will see that the reflux of the Thames, as far as Windsor, is caused by the rising of the tide and the agitation communicated to the river. Notwithstanding that Windsor is many leagues from the sea, the influence of the tide on the interior waters is regularly felt. In the same manner the Mediterranean and the Red Sea will agitate the Suez Canal.

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[1] *Vide* "A Letter addressed to the Editor of the 'Austrian Gazette,'" page 5. London, 1858. (Tract, 8vo., vol. 112.)

The waters will rise and fall; in one word, they will take part in all the movements of the sea. The canal, I repeat, is merely an elongation of the two seas to their point of union in the Bitter Lakes, and it will always be fed by them."

Mr. Robert Stephenson thus replied to M. de Negrelli's statements:—

"If 'my honourable friend' will stand on the walls of the city from which he has developed 'such singular hydraulic knowledge,' he will see in the tideless stream below him nothing of the character he describes as occurring in the Thames, which will probably account for his statements, which are indeed as wide of the mark as possible. Because to suppose, for a moment, that there is any analogy between the Suez Canal, 300 feet wide at its mouth, in an almost tideless sea, and a river like the Thames, no less than 6 miles broad at the Nore, with a rise of tidal water of from sixteen feet to twenty feet, is really, to use a favourite phrase of M. de Negrelli, merely to 'PRETEND' to a knowledge of hydraulics."

And if 'my honourable friend' is wrong in his analogy, he is equally unfortunate in his special illustration. If, without a shadow of foundation, M. de Negrelli questions my ever having been at Suez, it is not without foundation that I shall question his ever having been at Windsor. Or if ever, as his language appears to suggest, he has travelled up the River Thames, past the windows of the building in which I 'developed such singular hydraulic knowledge,' to visit the Royal Castle at Windsor, from whence he saw 'the reflux of the Thames, the rise of the tide, and the agitation communicated by the river,' he must forgive me for suggesting that the vision must have been under circumstances most dazzling to his clear perception. For, whilst it is quite true, as M. de Negrelli says, that 'Windsor is many leagues from the sea,' it is equally certain that 'the influence of the tide there is' NOT 'regularly felt,' inasmuch as it is arrested by Teddington lock. Moreover, I must inform M. de Negrelli, that Windsor is twenty-six miles above the reach of the tide of the Thames; and that at no period of the history of the river, even before the construction of the locks, was the tide known to reach within twenty-two miles of that town. In the same manner therefore as the tide agitates the waters at Windsor, M. de Negrelli pronounces that 'the Mediterranean and the Red Sea will agitate the Suez Canal.' On this point, 'I do share the opinion of my honourable friend in'—Austria.

"But on the graver question that 'the waters of the Canal will rise and fall, that they will take part in all the movements of the sea,' I 'do not share his opinions.' Supposing that there was an action of the water to the extent of two feet in the Mediterranean, and of six feet in the Red Sea, which is supposing an extreme case, what effect would such action have on a canal nearly eighty miles in length, with the intervention of the extensive basin of the Bitter Lakes? The matter is easily to be settled by calculation; and I leave it to my honourable and ingenious friend to amuse himself with it in his leisure hours."

Mr. Stephenson then commented upon a document issued by the Hydrographic Office of the Admiralty, under the authority of Captain Spratt, R.N., who, in 1857, made 'An Inquiry into the soundness of M. de Lesseps' reasonings and arguments on the practicability of the Suez Canal:'—

> "This eminently scientific officer has arrived at the conclusion, after twenty years' hydrographic experience of the Mediterranean, and after the late elaborate surveys and charts of the Admiralty, showing the wave-motion and the currents, that the establishment of a Canal, 'dependent upon, or secondary to, the practicability of making and maintaining a deep entrance to it from the tideless Mediterranean,' is not feasible. 'It is necessary and just,' he concludes, 'to the commercial interest, desiring to embark in the project of M. Lesseps, to know these important facts, ere they risk their millions in the undertaking, instead of discovering them only when swamped in the sands they will have to contend with; and then to discover also, when too late to amend it, that Alexander the Great was wiser than M. Lesseps admits, when he listened to the local opinion regarding the influence of the Nile upon the harbour, if formed to the eastward of it.'"

He would, finally, direct attention to the fact, that Mr. Stephenson was a warm supporter of the Suez Canal project until it was ascertained, beyond a doubt, that the difference of level of 30 feet between the two seas did not exist—that he had actually expended the sum of £1,500 out of his own means to have positive information upon which he might form his opinion as to the practicability of the Suez Canal measure; and in the letter from which he had quoted he also said:—

> "In conclusion, Sir, I will only say that I have—indeed, I can have—no hostility to a Maritime Canal through the Isthmus of Suez. If I could regard such a Canal as commercially advantageous, I have already shown that I should be the first to give it the advantage of my time, my money, and my experience. It was because, after elaborate investigation, and in conjunction with such men as M. Talabot, I arrived at a clear conclusion that the project was not one which deserved serious attention, that I refused to give it support. I should be delighted to see a channel like the Dardanelles, or the Bosphorus, penetrating the Isthmus that divides the Red Sea from the Mediterranean. But I know that such a channel is impracticable,—that nothing can be effected, even by the most unlimited expenditure of time, and life, and money, beyond the formation of a stagnant ditch, between two almost tideless seas, unapproachable by large ships under any circumstances, and only capable of being used by small vessels when the prevalent winds permit their exit and their entrance. I believe that the project will prove abortive in itself, and ruinous to its constructors; and entertaining that view, I will no longer permit it to be said, that by abstaining from expressing myself fully on the subject, I am tacitly allowing capitalists to throw away their money on what my knowledge assures me to be an unwise and unremunerative speculation."

Mr. J. M. Heppel said, with regard to the currents from the Mediterranean Sea and the Red Sea towards the Bitter Lakes and vice versâ, that he understood Mr. Hawkshaw's meaning to be, that the water in the middle held itself to about the mean level of the tides on either side, and that as they alternated there would be, from or towards either end, reciprocating currents. If there was any great amount of evaporation from the middle, that would no doubt create a certain difference in the amount of current in and out; but that difference would be small compared with what caused the main currents, which were simply due to the fact that the water in the middle would stand at the mean level of the tides on both sides of it, and that as they rose and fell alternately corresponding currents would take place.

Mr. Murray remarked, that every Engineer must for many years past have paid attention to the subject of a canal uniting the Red Sea and the Mediterranean. If it was possible to carry out a ship-canal between the two seas, it would be an immense boon to the trade between Europe and the East. But as to the practicability of executing the scheme as at present developed, he had many doubts, arising from what had taken place by natural means in the course of time. At the mouth of the Nile there existed an immense Delta, extending from the old Pelusiac mouth to Alexandria, a distance of 160 miles; and from a point on the Mediterranean, halfway between Damietta and Rosetta, to the fork below Cairo, it extended a distance of over 90 miles. The great bulk of this area of upwards of 7,000 square miles in extent had, no doubt, accumulated over a very lengthened period. If one-third of it were taken as water, then nearly 5,000 square miles of land had been formed between Pelusium, Alexandria, and Cairo. That large area had been produced from the deposits of the Nile and the drift of the sea. Further deposits were now taking place along the seaboard, between Alexandria and Pelusium, indicative of this action; and as the prevailing winds from the north and north-west struck the shore, the currents carried the detritus from the mouths of the Nile in an easterly direction. The immense tracts of sandy desert at no great distance from the African shore of the Mediterranean, would seem to indicate a higher level of the sea than there was at the present day. If that were so, there must have once existed a communication by water between the Mediterranean and the Red Sea. That such a communication did exist was corroborated by the existence of the waters of Menzaleh, Timsah, and the Bitter Lakes; and further, by the remains of the ancient Canal. It was the general opinion that this work was wholly artificial; but he conjectured it was only the transformation of the ancient channel between the Mediterranean and the Red Sea into a navigable canal. Any communication between these seas at different levels must have produced currents and a bar, leaving the central part occupied by the Bitter Lakes as the lowest portion. The bar, or more than one, having closed the communication, the sands of the desert had taken possession of the high ground at El-Guisr, and other places.

Now he thought the Canal proposed by M. de Lesseps was only returning to the old state of things. It was meant to open an uninterrupted communication between the Mediterranean Sea and the Red Sea without locks. If that were accomplished, a bar would unquestionably be formed in the new Canal, as it had been formed in the old water-course. He thought that it was not correct engineering to allow water to be admitted into this Canal with currents flowing towards the Bitter Lakes from the sandy shore of the Red Sea, and from the sand and fine mud composing that of the Mediterranean. He had prepared a diagram of the tides, taken from the table given at page 232 of the Report of the International Commission, which again was compiled from levellings made in 1847, at the instigation of MM. de Negrelli, Robert Stephenson, and Talabot, by M. Bourdaloue. A verification of these levels was again made in 1853, and gave as the difference between the station on the quay of the hotel at Suez, and low water in the Mediterranean, 2·4286 feet, instead of 2·6100 feet, found by the operations of 1847. It appeared from this diagram (Plate 14^{A}) that the mean level of the Red Sea was 2 feet 6 inches above the mean level of the Mediterranean. The water rose and fell a little in the latter as the north or south gales blew on or off the coast, giving an extreme range of about 3 feet 7 inches. But in the Red Sea there was a greater difference of range, viz., at neap tides, 2 feet 7 inches; at spring tides, 5 feet 3 inches; and at equinoctial spring tides, when accompanied with violent southerly and northerly winds, of 10 feet 7 inches.

Looking at the situation of the Bitter Lakes, and their extent, if it was intended simply to dredge a channel through them, and embank each side of the Canal through Lake Menzaleh, as reported by Mr. Hawkshaw in 1863, then the whole area of these Lakes must be filled either from the waters of the Mediterranean or from the Red Sea. With the transverse section, produced by Mr. Abernethy as the one now adopted throughout the length of the Canal, he had considered the question of filling the Bitter Lakes, which had an area of about 157 square miles. The evaporation in twenty-four hours might be taken at between 6,750,000 cubic yards and 10,000,000 cubic yards. The supply of this large body of water could only come from the Red Sea; as the length of the channel from the Mediterranean of 53 miles with the small rise of tide was quite inadequate to admit water from that end, if the outward current were taken into consideration.

If the water in the Bitter Lakes were assumed to stand at the level of low water of an ordinary spring tide in the Red Sea, then the quantity capable of running through the channel of 13 miles in length in the period from low water to high water, and from high water again to low water, or in twelve and a half hours, could only be 10,300,000 cubic yards, without taking into account the evaporation from 13 miles of channel. If again the water in the Bitter Lakes were assumed to stand at the level of low water of a neap tide in the Red Sea, then the quantity capable of running through the channel for a similar period could only be 6,500,000 cubic yards. It was evident, therefore, that, during great evaporation, the water in these lakes must settle to a level below the low water of a spring tide in the Red Sea, and cause the current inwards to be nearly constant, of course attaining a greater velocity at the periods of high water; or, as it appeared to him, the sectional capacity of the channel was much too small to supply the evaporation from the Bitter Lakes. The evaporation also could not be neglected in so warm a climate in a total length of 90 miles of channel, with a breadth varying from 267 to 328 feet. The supply, therefore, of these Bitter Lakes with water appeared to him a matter of great difficulty, as well as the upholding the channel between them and Suez, which would be liable to be acted on by the force of the current during high water of equinoctial spring tides.

He had assumed the evaporation to be from half an inch to three-quarters of an inch in twenty-four hours. The late Mr. Joseph Gibbs (M. Inst. C.E.), told him he had made experiments at Suez, but the evaporation was a great deal more than three-quarters of an inch per day in hot weather.

The next point to which he would direct attention was as to the probability of a bar being formed at Port Saïd. Mr. Abernethy had stated that the pier was now run out to a length of 1,500 mètres from the shore. The drift being almost constant towards the Levant, there could not be a doubt that this mole, or breakwater, must intercept the current, and arrest the detritus in its course. It might take some years to cause a large deposit, but the material being sand and fine mud, would readily be acted on by on-shore winds, and the sweep of the sea would carry it at such times round the head of the pier, and form a bar between it and the head of the eastern jetty. That bar could, of course, be kept low by continual dredging, at considerable expense. The direction given to the eastern jetty was an improvement upon the original design of a parallel pier; but he thought it would have been better if it had been more splayed and isolated, so that the deposit, if it did occur, would be kept from accumulating by being swept onwards with the current. Then he thought, looking at the large extent of Lake Menzaleh, consisting of nearly 600 square miles, it was a great pity so large a basin was not made use of, for scouring purposes, on the principles so successfully adopted by some of the French Engineers. By admitting the high waters of the Mediterranean, by means of tumbling bays placed in proper positions in the barrier between Port Saïd and Damietta on the one hand, and Port Saïd and the Pelusiac mouth on the other, the impounded waters of Lake Menzaleh might then have been led by culverts with sluices at their mouths, placed in the western jetty, and at the entrance of the new basin, and thereby have been the means of sending out at low water into the Mediterranean an ample current with good effect upon the deposits formed within the heads of the jetties.

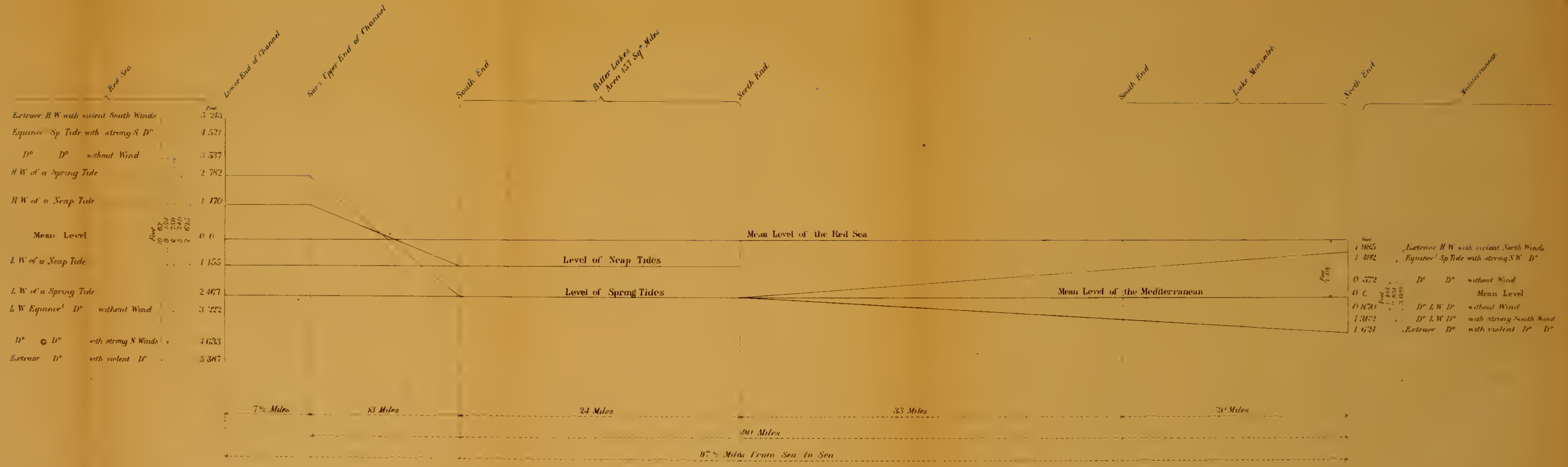
The conclusions at which he had arrived were these:—if the Bitter Lakes were allowed to exist and be fed with water, it enforced the necessity of locks at each end of the Canal, to prevent the constant indraught and consequent deposit within it. By means of these locks the height of the water might be regulated in any manner thought best. He considered further, the system, for supplying the maritime Canal, proposed by the late Mr. Rendel, who formed one of the members of the original International Commission, should have been adopted; viz., by maintaining the waters of the Canal about 23 feet higher than was now proposed, by fresh water of the Nile derived from above Cairo. The fresh water canal would, in that case, have been made some miles longer, and probably larger in section. Other advantages would have resulted from this plan, the chief of which would have consisted in reducing the excavations nearly one-half, a most important point in the cost of the works, and in reducing the period of their execution.

The Astronomer Royal said his connection with these matters was rather of a literary and philosophic than of an engineering character. It was a question which in all points he looked upon with great interest. He would ask in the first place, as a point which he did not think affected the engineering question materially, but which was one of considerable interest in matters of tidal phenomena, and in regard to the supply of water to large marine gulfs generally—What was the evidence of the alleged difference of levels in the two seas? He might state that he asked this question because he was at one time tolerably familiar with the work in French which was drawn up by the joint commission of Engineers, of whom the late Mr. Stephenson was one; and the impression he derived from that was, that after correcting the enormous errors in the surveys which were made during the French invasion in the beginning of the present century, after going through the whole line, they found no perceptible difference in the mean levels of the two seas. He would be glad to be certified whether there was in fact a difference of 2 feet 6 inches between the mean levels. It was conceivable as a physical fact.

Sir W. Denison said he was assured by the French Engineers on the works that the mean level of the two seas was the same.

The Astronomer Royal: In the next place, with regard to the evaporation of the water from the Bitter Lakes, there was a point of history about which there was no doubt; namely, that for a long time the Bitter Lakes were filled with sweet water. Whatever the supply was then, he thought there might be as good now; so that on the question of evaporation he had no fear. In the third place there was a point as to the intention of the ancient Canal. There was

SUEZ CANAL.


Red Sea
Lower End of Channel
Suez Upper End of Channel
South End
Bitter Lakes Area 151 Sq. Miles
North End
South End
Lake Menzaleh
North End
Mediterranean
Feet
Extraor. H.W. with violent South Winds 5 243
Equinox Sp. Tide with strong S. D° 4 521
D° D° without Wind 3 537
H.W. of a Spring Tide 2 782
H.W. of a Neap Tide 1 470
Mean Level 0 0
Feet 10 63 9 154 6 759 5 249 2 625
L.W. of a Neap Tide 1 155
L.W. of a Spring Tide 2 467
L.W. Equinox¹ D° without Wind 3 222
D° D° with strong N. Winds 4 633
Extraor. D° with violent D° 5 387
Mean Level of the Red Sea
Level of Neap Tides
Level of Spring Tides
Mean Level of the Mediterranean
Feet
1 985 Extraor. H.W. with violent North Winds
1 492 Equinox¹ Sp. Tide with strong N.W. D°
Feet 1 316
0 372 D° D° without Wind
0 0 Mean Level
Feet 1 242 1 861 3 608
0 870 D° L.W. D° without Wind
1 362 D° L.W. D° with strong South Wind
1 623 Extraor. D° with violent D° D°
7½ Miles
13 Miles
24 Miles
33 Miles
20 Miles
90 Miles
97½ Miles from Sea to Sea

abundant evidence on the spot that there had been canal communication from the Nile leading into the sea at Suez by the way of the Bitter Lakes; but it had been the opinion of some persons that this canal was made for navigation by vessels, while others were of opinion that it was made for a totally different purpose. Those who had studied the circumstances of Egypt under the floods of the Nile would be acquainted with the fact, that the great floods of the Nile were far more destructive than the small floods. The thing most dreaded in Egypt was the excessive flooding of the Nile; and it was supposed that that canal had been made to assist in the discharge of the excess of water into the Bitter Lakes. Those who had been on the spot would be able to judge better on that point. The circumstances attending the formation of a canal at the south-east end and the north-west end must be different. He gathered from the French account he had spoken of, that though there was some sand at the south-east end, it was of no great importance. The traces of the original canal were perfect, and they were found to be not through sand but through rock, and there the canal was in place with its original sides and bottom, and the rubbish thrown out at the sides; so that in all respects that part would appear more easy to make, and would be more durable when made, than the part at the other end. So far as the rush of water was concerned, arising from the great tide at Suez, in consequence of the hardness of the ground he should think there was less to fear than if there had been such a tide at the other end. Finally, on the engineering question, M. Cialdi's name had been mentioned; he had seen the report which had been made by him, and would ask whether the plan he suggested had been executed or was contemplated. The plan was, that when the long western pier was carried out, it should have a considerable return to the west, somewhat like the Folkestone harbour pier; and that being constructed, a space should then be left vacant, and another detached pier should be built, in the continuation of the line of the long western pier. That was the plan which accompanied M. Cialdi's report.

Mr. J. B. Redman said he had not been on the site of this work, but the interest attached to it was universal. Sixteen years ago a Paper was read before the Institution by Mr. Glynn,[1] upon the schemes which had up to that time been brought forward for canal communication between the Nile and the Red Sea. At that period there was an assumed difference of level between the Red Sea and the Mediterranean exceeding 30 feet at high water; but in the discussion which followed, Mr. Robert Stephenson showed the error in the levels, and by reference to a series of levels carried out under his direction, he assumed that the mean low-water level of the two seas was identical. The Author of the Paper, now under discussion, assumed that the levels were the same, supposing the levels of the French Engineers to be correct. Without referring to levelling operations one would have imagined that the two levels were mean levels of the sea, and identical; but he thought reference should be made, in respect of tidal observations, to the mode by which that fact had been determined. With regard to the work itself, it occurred to him that there were several points of interest which had not been much touched upon. There was the question of keeping open the channel between the two seas; that of the slopes of the canal; the embankment through the Bitter Lakes; the slopes external as well as internal; the cost of dredging operations compared with manual labour; and the pier at the Mediterranean entrance. The Author of the Paper assumed that the slopes of the Canal would have to be increased from 2 to 1 to 6 to 1. That was a gigantic work, and would involve a very considerable addition to the estimates, because it amounted to setting back the banks to a great extent. It was argued by the Author, that that slope was essential, to prevent injury to the banks, from the wash occasioned by the passage of vessels; but it occurred to him that it was hardly necessary to increase the slope to so great an extent, although protection should be given to that portion of the slope, which was affected by the wave from the vessel, by some artificial means. But he understood from gentlemen who had been over the route, that a more serious question was involved in the embankment through the Bitter Lakes; that was, assuming the Canal to be embanked through the Lakes, the question of protecting the lake slopes.

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. x., p. 369.

Mr. Hemans observed that there would be no embankment through the Bitter Lakes. Lake Menzaleh would be embanked.

Mr. Redman said that disposed of the question at once. With regard to the pier at the Mediterranean entrance, the cost given for that work showed it was a very expensive one. It was thought that, as there was an existing drift towards the east, the material would be arrested by the pier, and subsequently be carried to the leeward. Undoubtedly the effect of that pier would be to arrest the drift within the length of its projection. There would be a deposit to windward, but there could no longer be a drift to the leeward of the pier within its projection within certain limits; consequently a certain amount of deposit in the dead water must be anticipated. With reference to the comparative cost of dredging and manual labour, the results stated by the Author showed, that the excavation could be more economically performed by hand labour. In this country it was found that dredging, under the most adverse circumstances, was the more economical operation, being something like one-third of the cost of hand labour. But in this case the application of machinery occurred under disadvantageous circumstances, where repairs from accidents were costly, while manual labour in many respects approached the lowest order of coloured labour. The whole subject was replete with interest; but he confessed, as far as the Paper and the discussion it had elicited went, it appeared to him a very speculative matter.

Mr. Hawksley, V.P., said he should like to correct, what he conceived to be a material error, with regard to the amount of evaporation, and consequently with regard to what had been alleged as to the impossibility of filling these lakes. It was known that, in India and other tropical countries, the actual amount of evaporation did not exceed from 6 feet to 8 feet per annum, instead of being, as was supposed, an inch per day, and therefore amounting to 30 feet per annum. That was during the dry season, for during the monsoons the evaporation was not felt to the same extent. It was quite plain, if he was right in what he ventured to state was about the fact, that the evaporation did not amount to one-third of an inch per day. The probability therefore was, that the indraught of the Bitter Lakes would not give an average velocity of more than 1½ foot per second during the time the tide was above its mean level, and of course the outdraught would not be so considerable; consequently on the whole it might be fairly assumed, that the level of the Bitter Lakes would be maintained by an indraught having a velocity of 2 feet per second, which would not be a serious obstacle to navigation.

The outdraught into the Red Sea would not be so considerable, because it would cause a declivity in the channel of the nature of a 'round back,' and prevent the water coming out of the Lake with great velocity. That it would draw some water from the Lake there could be no doubt; but that any considerable amount would be drawn out he did not believe. The Lake would stand permanently, no doubt, at a higher level than was shown in the drawing, and at a mean level lower than the Red Sea and higher than the Mediterranean. With regard to the level of the Mediterranean he would make this observation: he believed it was perfectly true that the level of the Mediterranean was lower, and must necessarily be so, though not to a great amount, than that of the Red Sea; because the evaporation from the immense surface of the Mediterranean and the Black Sea—the area of which was eight or ten times as great as the area of the Red Sea, whilst the sections of the mouths were not nearly so dissimilar—was greater than the discharge from the rivers which came in to supply those seas. Consequently, except when there was a disturbance from gales, there was always a strong indraught from the Atlantic into the Mediterranean, which there would not be unless there was a fall from the Atlantic into the Mediterranean; and it therefore followed that the level of the Mediterranean must be lower than the level of the Atlantic and Indian Oceans, with which the Red Sea communicated. The amount was probably less than the 2 feet 6 inches which had been determined by recent levellings as the difference of level between the two seas.

Mr. Brooks remarked that the difference of rainfall in India and Egypt should be considered.

Mr. Hawksley did not think that had much to do with the question. If there was an evaporation of 30 feet per annum from the ocean, there must be a rainfall of an equivalent amount somewhere, and in tropical regions a rainfall of 30 feet over an area equal to that of the tropical seas was improbable, and, in fact, no such rainfall occurred; but there were rainfalls which were quite as great as the amount of the evaporation which took place from the deep lakes, and even the deep tanks in India. The experimental evaporation from a shallow vessel would be more rapid than from the surface of a large extent of deep water, because the rays of the sun penetrated immediately to the sides and bottom of the vessel and increased the evaporation. That was the case in all experiments made in England, as well as in those which were reported to have been made in tropical climates.

Mr. Cockburn Curtis stated, that during the period he was employed in the Public Works Department of the Madras Presidency, it was customary to assume a quarter of an inch per diem as the average evaporation throughout the year from large areas of water varying from 3 feet to 6 feet in depth. The observations of Lieut. Ludlow, M.E., at the Red Hills, in 1844,[1] which extended from April to August in a comparatively dry district, and during the hottest and driest months of the year, gave a maximum on one occasion of ·6011 of an inch per diem, and an average of ·3745 of an inch per diem. The amount of evaporation would in all cases be found to be in inverse ratio to the depth and volume of the water experimented upon.

Sir William Denison said he congratulated himself upon having been the cause of such a discussion as had taken place. It had brought before the Engineers of England a state of things in Egypt of which not very much was known, but which had at various times excited a great deal of discussion and much difference of opinion. Some of these opinions were of a purely hypothetical character, and had no reference to the works which were now under consideration; as, for instance, that a fresh-water canal at a high level, 21 feet above the sea, supplied from the Nile, and dropping by locks into the sea at both ends, would be more easily made and maintained than the proposed salt-water cut between the two seas. He did not think that it would be possible to make, out of the soft mud of the Delta, embankments capable of resisting the thrust of a head of water of 21 feet; but it was useless to discuss such a matter of opinion. He had brought the subject forward in order to draw attention to certain facts with which he had become acquainted in passing through the country, and to elicit an opinion upon them.

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[1] *Vide* "Reports, Correspondence, and Original Papers, on various Professional Subjects connected with the Duties of the Corps of Engineers, Madras Presidency," vol. iv., page 131. Madras, 1856.

One of the questions was with reference to the sectional area of the Canal. It struck him that it was too narrow, that the banks were too steep, that they would not stand at the slope given; and it appeared that since he left the country, a modification had already taken place. The French Engineers had diminished the bottom width, which was 83 feet, and had made it 72 feet. They had increased the slope of the banks from 2 to 1 to 3 to 1. They had widened the foreshore, and had increased the width at the top from 250 feet to 328 feet. Therefore, to a certain extent, his own opinion had been corroborated. He fully admitted the correctness of some of the observations that had been made. He did not, of course, imagine that the slope of 6 to 1 would extend uniformly from top to bottom. The slope would assume a curved form, there would be 1½ foot or 2 feet of bank above the water line, and upon that the wash of passing steamers would act. It would bring down silt, and the angles of the foreshore would be filled up. Portions of the upper part of the slope would be cut off. There would gradually be a deposit at the bottom, and the surface of the cutting would be rounded off by the action of the water put in motion by the large bodies which would pass through it. He had supposed the average of the vessels to be 2,000 tons, with 40 feet beam, and 20 feet draught of water; and he thought it would be found desirable, indeed, absolutely necessary, to widen the bottom of the canal. If two vessels met, unless they were in actual contact, the keel of one or the other would take the ground upon the bank; more particularly would this be the case if the now proposed width of only 72 feet should be carried out. He believed that, eventually, it would come pretty nearly to what he said, that the Canal would be much wider at the top than was now shown, and that the banks would be more sloped. It must be recollected that this work was done with dredging machines, and the slopes would be very roughly cut, and in steps; that the edges of these would be softened and worn away, and would gradually go to the bottom; so that bit by bit would be dredged out till the slopes assumed a curved form. The expense of this would be spread over a number of years, and would come under the head of what the French had estimated for maintenance, and keeping the channel clear of the silt. He had said there must be sand or dust from the desert. He happened to be there when the Kamsin, or hot wind, was blowing, and he could not see 20 yards before him. There was a great quantity of dust from the dry soil, because, though Lake Menzaleh looked, upon the plan, a very fine piece of water many miles long, yet it was only at best a few inches deep, and it never had any water in it except when the wind blew from the north-west. At other times it was positively dry mud, and the dust was raised from the dry surface of that mud; but as he had said, he believed that would be a temporary evil only. They were lessening it now by planting on the bank, and putting up fences, and he did not see that there was anything to prevent the Engineers fixing the sand gradually, by appliances which would in time be devised; at all events the evil would be much lessened, although the maintenance for the first ten years might be heavy, but in the end the Engineers would reduce it to a fair average amount. He agreed with Mr. Hawkshaw that the Canal was but a large ditch; and that it only required money and a moderate amount of engineering and mechanical skill to perfect it. Allusion had been made to the money part of the question. In the first place, it must be recollected that the Company was formed under the idea, that they would have a claim on the Egyptian government for the supply of an amount of forced labour to the extent of twenty thousand men, with the grant of a large quantity of land along the banks of the Canal. Political objections were made to that, and the Pacha was prohibited both from supplying the forced labour and from granting the land. Then the Company, of course, went to the Egyptian government for compensation; and he had heard, and he believed it to be correct, that the Emperor of the French, who acted as umpire, gave an award of £3,500,000 as compensation; so that, if that sum were added to the capital of £8,000,000, it would amount to nearly 50 per cent. of the whole capital, and he did not suppose the Pacha would be entitled to interest upon that £3,500,000. He was told that they were working upon that, and it was a handy sum to work upon. Roughly speaking, he estimated that there would be nearly double the amount of excavation that was calculated, and, at the rate they were proposing to dredge, it would take much more time than what had been stated: he believed it would take four or five years instead of three. With regard to the economy of dredging, on one occasion he watched the dredging machines at work, cutting the soil, and bringing it to the bank as had been described. As long as the scoops brought up half water and half mud, the operation went on very well. There was then sufficient water to carry the mud down the metal spout; but when the machine brought up soil only, it stuck in the spout, and the action of the dredge was checked, and it had to bring up water only, while four men were employed to make the stuff move in the spout. At the time he was there they were dredging only to the depth of 7 feet or 8 feet; but when they got to a depth of 20 feet or 25 feet, there would be a great difference in the lift of the stuff, and the distance it had to be carried away. He, however, was not in a position to deal with the question of absolute expense, nor did he pretend to do so. There was one more point of importance, that was, the question of the harbour at the Port Saïd end. He was glad to hear it had been carried out to so great an extent during the last twelve months, since he was there; at that time the work only projected some few hundred yards into the sea, a portion of which only was silted up; but now it appeared there was a length of 1,500 mètres above water. It was evident they were working very hard at it, and he saw no reason to question the possibility of completing the work to the full extent of 3,000 mètres, which would bring it out to the depth of 4½ fathoms.

Then came the question of the drift along the shore. It was well known that there was a steady drift along the south coast of England. That was not caused by a littoral current. There was no current along the shore sufficiently powerful to move the shingle, but the motion was occasioned by the oblique action of the waves; by this the shingle was drifted along the coast eastwards. It was not the effect of the current, but of the waves on the beach, and at a very moderate depth there was no movement of the shingle at the bottom at all. The same cause was operating, in exactly the same manner, on the coast of the Delta.

With regard to what the Astronomer Royal said about the remains of the ancient Canal, he could only say that a muddy hollow was pointed out to him as a portion of it; into this the water from the excavation at Chalouf was being pumped, and there were rushes and weeds growing there, which would not have been the case if it had been a salt-water channel. As to the geology of the locality, there was a rise of 12 feet or 14 feet at Chalouf above the Red Sea, composed of alternate strata of rock and sand, and this was evidently a spur from the range to the west of the Canal; the date of the elevation of this, however, he would not attempt to decide. He had no doubt the Astronomer Royal was right in stating that it was originally a fresh-water canal: how the Bitter Lakes became salt was more difficult to say. He did not see how the Canal could become a stagnant ditch, since it was open to the sea at both ends, with, in one case, a difference of level of 8 feet between high water and low water. There must of necessity be a strong current as far as the Bitter Lakes on that side, while the amount of evaporation, which he took to be at least 9 feet per annum, or from ¼ to ⅓ of an inch per day, together with the slight tide of the Mediterranean, would maintain a sufficient current to prevent anything in the shape of stagnation.

He had not visited the Bitter Lakes, but he was told they were dry, with the exception of the water which was brought in for the purposes of the work. With regard to the tide in the Mediterranean, he imagined the rise and fall of 8 inches or 9 inches must result from a tidal influence of some description, as it was of daily occurrence; but he could not say that it took place at the same hour of each day. The Engineers there told him there was something analogous to a tide of about 9 inches.

Mr. Hawkshaw wished to explain, in reference to the currents which he calculated would obtain in the Suez Canal, that he considered they would be caused mainly by the rise and fall of the tides. There was a rise of tide of 3 feet 6 inches in the Red Sea, which must necessarily produce a current into and out of the Canal. The evaporation of the Bitter Lakes would increase the inward current to some extent, and cause it to exceed the outward current. But the main cause of the current would be the rise and fall of the tide.

In pursuance of the notice on the card of the Meetings, it was proposed, and resolved unanimously:—

"That in order to insure a fuller attendance of Members than could be obtained on Easter Tuesday, the Meeting be adjourned until Tuesday evening, the 30th of April."

April 30, 1867.

JOSEPH CUBITT, Vice-President, in the Chair.

The discussion upon the Paper, No. 1,172, on "The Suez Canal," was continued throughout the evening, to the exclusion of any other subject.

May 7, 1867.

CHARLES HUTTON GREGORY, Vice-President, in the Chair.

The following Candidates were balloted for and duly elected:—Harry Footner, and William Aubone Potter, as Members; Henry Slingsby Bethell, Armand Bouquié, George Brown Murdoch, Joseph Smith, and Henry Waugh, as Associates.

No. 1,180.—"On Optical Apparatus used in Lighthouses."[1] By James T. Chance, M.A., Assoc. Inst. C.E.[2]

The following notes are designed to convey a general idea of the chief contrivances which constitute the existing system of Lighthouse Illumination, and to trace the steps of their development.

The subject is of great practical importance, and furnishes an interesting application of optical science.

A complete sketch of Lighthouse Apparatus would far exceed the due limits of this Paper; and, moreover, the various questions connected with it have been systematically treated by the late Mr. Alan Stevenson and by Mr. Thomas Stevenson, M. Inst. C.E., to whose works the Author is indebted, as likewise to the following French sources, namely: 'The Mémoire of Augustin Fresnel,' which was read at the Academy of Sciences in July, 1822; 'The Report of the French Lighthouse Commission,' dated September, 1825; and the recent 'Mémoire of M. Léonce Reynaud,' Director of the French Lighthouse Service.

The object of Lighthouse Optical Apparatus is to condense, within a small equatorial zone, the available part of the rays which diverge in all directions from a given source of light; so that as much of it as possible shall be rendered serviceable to the mariner, in the most effective manner, compatible with the special conditions of each locality.

The ordinary source of illumination is the flame of an oil lamp on the Argand principle. A single cylindrical wick is employed in the small harbour lights of the dioptric construction, and also in nearly all kinds of apparatus which consists of metallic parabolic reflectors. But in dioptric sea lights the burner comprises two or more concentric wicks, four being used in the lamp which belongs to an apparatus of the first order; and as this arrangement necessitates a considerable superabundance of oil beyond what is wanted to feed the flame, various methods have been adopted for producing the requisite uniform supply. The oil generally employed is the colza; in the lamp, however, with a single wick, it is being superseded by petroleum, which is cheaper and gives a more intense light than colza.

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[1] The discussion upon this Paper occupied portions of three evenings, but an abstract of the whole is given consecutively.

[2] The Author was elected Assoc. Inst. C.E., May 21, 1867.

The magneto-electric spark has been successfully applied to lighthouse purposes, and it bids fair to be ultimately adopted at most of the important lighthouse stations which are ready of access and otherwise suitable. This brilliant source of light has been continuously used at Dungeness by the Trinity Board since the autumn of 1862, the magneto-electric machine being that of Mr. Holmes. The following remarks, however, upon lighthouse apparatus will refer mainly to the oil lamp as the source of light.

THE DIOPTRIC SYSTEM.

The Optical Apparatus which is now being universally adopted for sea lights is of the dioptric kind, first successfully introduced by the eminent Augustin Fresnel.

It consists of a structure of glass zones, or segments, which in a complete apparatus envelopes the sphere of light radiating from the central flame, except that portion which is intercepted by the burner or is occupied by its chimney. Fig. 1,[1] Plate 15, which is in a plane of the vertical axis of the system, represents the sections which generate the successive zones, and which are such that all rays diverging from the principal focus are made to emerge in a horizontal direction. The vertical axis of the burner coincides, of course, with that of the apparatus. In reality the upper, middle, and lower portions of the system have generally different foci. An angle of about 57°, which the focal horizontal plane bisects, is acted upon by refraction alone; but the rays which pass above and below this angle are deflected by internal total reflection.

The generating sections may evidently describe zones, either round the vertical axis, or round a horizontal one through the focus.

If the vertical axis be that of generation, then all rays from the focus will be parallelized only in meridian planes, and the natural divergence in azimuth will remain, so that an uniform light will be distributed to every point of the compass. This constitutes what is termed a Fixed Light.

If, however, the axis of revolution be a horizontal one, the action of the apparatus becomes lenticular, so that all focal rays will emerge parallel to the axis of generation, which will also be that of the compound lens. All the sections may describe complete rings round the horizontal axis, and this is done occasionally in small apparatus; but the usual method is to divide the sphere into segments, by meri-

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[1] The instrument represented in this figure is of the largest kind, and has a diameter of about 6 feet, and a height of about 9 feet.—J. T. C.

dian planes at equal angular intervals, the number of these divisions depending on the particular conditions to be satisfied, as to the recurrence and duration of the flashes. The interval adopted for most lights is that of 45°, as shown in Fig. 2. But whatever may be this angular division, each segment will send forth its own beam, in which all the focal rays will be parallel to the horizontal axis of revolution; and in order to render the series of separate beams serviceable to the mariner, the whole apparatus is made to revolve, so as to exhibit the appearance of an alternating succession of brightness and darkness, and hence is derived the designation, Revolving Light.

The flame has magnitude; and it is evident that on every point of the apparatus there is incident a conical beam of light, whose apex is that point, and whose directrix is the corresponding contour of the flame: and, if the ray passing through the focus be termed the axis of each individual cone, the axes of all the emerging beams will be parallelized, but the conical divergence will remain, though slightly modified, after transmission. This divergence can be diminished either by increasing the diameter of the apparatus, or by diminishing the size, and therefore the power, of the flame; but some divergence always remains, and is indeed indispensable both in azimuth and altitude for revolving lights, and in altitude for fixed ones. The difficulty in the former case consists in obtaining an adequate horizontal divergence without wasting light by useless vertical dispersion.

As the emerging light is always divergent, its intensity therefore, in any given direction, is subject to diminution in the ratio of the square of the distance.

The Revolving Light is evidently susceptible of much greater intensity than the fixed one, inasmuch as all the light abstracted in the revolving apparatus from the dark intervals contributes a proportionate increase of brilliancy. Thus in a first-order revolving dioptric light of eight sides, as the whole 360° are compressed into eight beams whose divergence is about $5\frac{3}{4}$°, the mean intensity of the flash will be about eight times that of the fixed first-order light. The intensity, indeed, of the brightest part of the flash in a horizontal plane, as measured by observation, is at least twelve times[1] that of the fixed light. In consequence, however, of the necessity of distinguishing lights, the fixed one, although so inferior in power, cannot be dispensed with. The diameter of the largest, or first-order apparatus, is rather more than 6 feet, and that of its quadruple flame is about $3\frac{1}{2}$ inches; the height of the flame above its blue portion being about equal to its diameter. As the flame is diminished in power and size, according to the requirements of the locality to be lighted, so does it subtend a less angle, and therefore the optical apparatus can be proportionately reduced. There are, accordingly, a second-order light, which has a flame with three wicks, and a third-order one, having a flame with two wicks. Then follows the gradation of harbour lights of different sizes, according to the power required.

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[1] It is assumed that the axes of generation of the upper, middle, and lower divisions of the apparatus coincide, or are in the same vertical plane.—J. T. C.

Now although the flame of a sea-light is large, the most effective part of it is comprised within a small compass, and subtends only a small angle at the centre of the lens. Again, the angle subtended in a meridian plane by the greater portion of the sea between the visible horizon and the lighthouse is also extremely small, so that practically whatever part of the flame sends light to the sea horizon is at the same time illuminating the chief range of the sea landwards. Thus, suppose the flame to be placed 300 feet above the sea, the distance of the horizon is twenty nautical miles; and yet fifteen miles from the horizon towards the lighthouse subtend only seventeen minutes, which angle corresponds to about $\frac{1}{6}$th of an inch at the axis of the flame in a first-order light. Hence the brightest sections of the flame, which correspond to the different parts of the apparatus, ought to send their rays to the horizon; that is, each successive zone ought to be both shaped and adjusted with such accuracy that the sea horizon focus shall be situated in the corresponding brightest section of the flame. This adjustment is now generally attended to, and for this improvement the mariner is indebted in a great measure to the late Royal Commission; but in consequence of the prevailing misconception, that the size of the flame renders accuracy of shape comparatively unimportant, this latter desideratum is often neglected; and yet it is evident that if the middle of any particular zone be made to do its due work by means of adjustment, the whole of that zone ought to co-operate with its middle portion; and this can be effected only by the accuracy of its generating section.

This will be somewhat clearer when the subject is considered more in detail; but it must be manifest even from a general description, how immense must be the difference in power between one apparatus of which the parts are ground in conformity with theoretical accuracy of form, and which sends upon the sea only the brightest part of the flame, and another whose zones are so shaped that although the small middle portion may by adjustment be made to produce this effect, the remainder of it, perhaps, is sending the weak portion of the flame on the sea and the brightest part towards the sky, or else near the foot of the lighthouse itself. But this is not all; for as the axes of the emerging conical beams diverge instead of being parallel to each other, the light is diluted in every plane of the generating sections in proportion to this divergence.

The portion of the whole sphere which is embraced by the entire glass structure, after deducting the metallic framing, is about

81 per cent., which is distributed among the three divisions of the apparatus in the following proportions: the upper reflectors 22½, the refracting belt 45, and the lower reflectors 13½; but these ratios do not represent the actual relative illuminating values of the three portions. For several disadvantages appertain to the reflectors in comparison with the refracting division: first, the respective focal sections of the flame corresponding to them are weaker, and in the lower reflectors a degree of accuracy, scarcely ever yet obtained, is necessary to render effective the limited flame-section which sends its light to them; secondly, the longer paths described in the prisms involve greater loss by absorption; thirdly, the light which is transmitted by the reflectors has suffered more diminution by the greater obliquity of incidence, both at the two surfaces of the glass chimney and also at those of the prisms themselves. It is true that the longer focal distances of the reflectors, as compared with the refractor, are attended with a greater condensation of the emerging light; but the balance of these optical considerations is much in favour of the refracting portion, so that, as actual experiments seem to indicate, the relative illuminating values in the horizontal plane are approximately thus: for the refracting belt, 70, the upper reflectors, 20, the lower reflectors, 10.

Each zone, or ring, of the apparatus may have its own separate focus in the flame; but the general practice is to assign a common focal point to each of the three main divisions of the general vertical section, as shown in Fig. 1. Thus, while the focus of the refracting section is in the vertical axis, the upper reflectors have theirs at a short distance behind it, so as to combine with one of the most intense focal sections, corresponding to each prism, an adequate vertical angular range of light on the sea; and the focus of the lower reflectors is in the front of the flame, at the brightest section compatible with some amount of vertical divergence below the horizon-direction.

According to the usual plan now adopted, the lowest film of the brightest part of the flame is made to contain the sea horizon focus of the refracting panel; and then the reflecting zones or segments are so adjusted that their respective sea horizon foci shall be situated in the flame in positions which are in accordance with the principles just explained.

Too much stress cannot be laid upon the importance of selecting for the horizon, and sending towards it, through the various parts of the dioptric instrument, the corresponding brightest sections of the flame. The light ought to be visible to the approaching mariner as soon as the farthest horizon, which he can command, touches the horizon of the centre of the lantern; so that, in estimating the full optical range, the distances of these two horizons, from the lighthouse and the mariner respectively, must be added together. Now the emerging light, as has been already stated, is divergent, so that its intensity is subject to diminution in the ratio of the square of the distance; and there is a further loss of light, arising from the imperfect transparency of the atmosphere, which increases as the distance is augmented, though not in a direct ratio. Thus, in a clear state of the sky, each nautical mile abstracts from ordinary light five per cent. of the intensity with which it began to traverse that distance.

Let the intensity *in vacuo* at the end of the first nautical mile from the lighthouse be unity, then the respective intensities at successive miles in a clear atmosphere will form the series

$$\cdot 95 \;.\; \frac{(\cdot 95)^2}{2^2} \;.\; \frac{(\cdot 95)^3}{3^2} \;.\;.\;.\;.\;.\;.\;.\; \frac{(\cdot 95)^n}{n^2}$$

where n is the number of miles; and generally, if I be the intensity *in vacuo* at the distance of the first nautical mile, and p the proportion of the quantity of light absorbed by each mile, the intensity at the distance of n miles will be

$$\frac{\mathrm{I}\,(1-p)^n}{n^2}$$

When the atmosphere is hazy, the luminous range even of the brightest part of the rays is so limited, that a doubt may occur to some as to the expediency of directing the most intense light tangentially to the sea surface. But to rob the horizon of any light is to subject to the same decrease of illumination the chief sea range landwards, as has been before explained; and moreover, any increment of intensity thus obtained, even at a short distance from the lighthouse, will be scarcely appreciable in misty weather. If, however, it be desired to have a powerful dipping light, this should be provided by some accessory contrivance which will not interfere with the normal state of the main apparatus.

It is not intended to enter upon the various questions which concern the distribution of sea lights on a coast, and their adaptation to special localities. The solution of most problems of this kind requires not only a familiarity with the optical facilities which the dioptric system affords, but also a knowledge of the conditions which nautical experience supplies.

It suffices to remark that one chief difficulty which is encountered by the lighthouse Engineer consists in devising admissible characteristic distinctions among sea lights, subsidiary to the two grand divisions, fixed and revolving. He is occasionally forced to resort to colour; but the want of power in penetrating the atmosphere excludes generally all colours except red; and even in red colour the initial intensity is so reduced by passing through the colouring medium, that whenever it is employed in company with white light, special contrivances should be introduced into the apparatus in order to equalize nearly[1] the luminous intensities of the two kinds of light. This can be for the most part accomplished in the first instance in designing any particular instrument; therefore it is very important that any question of introducing coloured beams of light should be settled before the construction of the apparatus has been commenced.

A full account of the various modes of distinguishing lights will be found in the treatise of Mr. Alan Stevenson.

THE ANNULAR LENS OF AUGUSTIN FRESNEL, AND THE CYLINDRICAL REFRACTOR.

The Dioptric system will now be described in detail; and first, the annular lens of Augustin Fresnel.

No one can adequately appreciate the admirable combination of exact science with practical ingenuity which Fresnel displayed, in devising and carrying out in detail his annular lens and its accessories, without having perused his celebrated Mémoire which was read before the Academy of Sciences in July, 1822.

A Commission on Lighthouses had been appointed in France as early as 1811; and at the request of Arago, who had in 1813 joined the Board, Fresnel and Mathieu, a Member of the Institute, were in 1819 associated with him in conducting the necessary experiments and researches.

It is, indeed, creditable to the Administration in France that her highest men of science should be thus enlisted in the investigation of a national question requiring scientific treatment; and the result in this instance proved the wisdom of the selection. In September, 1822, the Commission confirmed an elaborate report, drawn up by Admiral de Rossel, in which Fresnel's system was adopted, and a programme was presented for the systematic lighting of the sea-coasts and harbours of France. This scheme was gradually carried into effect, and so strictly has it been adhered to, that out of forty-nine sea-lights which were proposed, only ten have been modified in their character, and the employment of metallic reflectors in sea-lights has been reduced to the single instance of a secondary lighthouse at Pontaillac, at the mouth of the Gironde.

Fresnel selected the annular form of lens, because, while it afforded the means of reducing considerably the substance of the glass, it also enabled him to give to each ring its own individual shape, so as to correct spherical aberration.

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[1] The word *nearly* is used, because red light, as might be anticipated *à priori*, loses a less proportion of its intensity than white light in passing through the atmosphere, especially in hazy weather; so that, with *equal* initial intensities, a red beam will have a longer luminous range than a white one.—J. T. C.

He seems to have been quite unaware both of Buffon's proposal, in 1748, to form a lens *à échelons* out of a solid piece of glass for the purpose of a burning instrument, and of Condorcet's valuable improvement, in 1788, of Buffon's idea, by suggesting that the burning lens should be constructed of separate rings. But, however this may be, Fresnel was the first to apply the lens effectively as a lighthouse instrument. His lens is plano-convex: he seems to have chosen this form chiefly for the sake of facility of execution; but it is also the best shape optically, for unless the angle subtended at the focus by the lenticular section be much diminished, concavity of the inner surface would render the external surfaces too oblique; and if the inner surface be convex, the angles of incidence in receding from the axis would be very disadvantageously increased; so that the total loss by reflection in each case would be greater than in Fresnel's arrangement, which assigns fairly to each surface its proper share in the total deflection required at each point.[1]

The only spherical surface in the lens is that of the central disc; the convex surfaces of all the encircling rings being annular ones, generated round the lenticular axis by circular arcs in the plane of that axis, but having their centres beyond it in a series of points which retreat further from the axis as each corresponding ring is increased in diameter.

The true generating arc for accurately parallelizing the rays from the focus is, of course, not a circular one, but its execution would be impossible; Fresnel, however, so calculated the co-ordinates of the respective centres of the actual arcs that the two extreme rays are made to emerge parallel to the axis. Now this approximation so nearly corrects aberration, that the greatest deviation, from the direction of the axis, of focal rays emerging from each of the successive rings varies in a diminishing progression from 2 min. 32 sec. for the ring next to the disc to 52 seconds at the eighth one.[2]

Fresnel at first encountered an obstacle in the optician's workshop, where none but the spherical form could be produced; rather, therefore, than lose time in his preliminary experiments, he composed each ring of small pieces having spherical surfaces indeed, but so calculated, in regard to curvature and obliquity, as to give the minimum mean aberration in all directions; and he also made it polygonal, in order still further to facilitate the execution.

His versatile genius, however, was not baffled by this temporary impediment; and he contrived expressly a system of grinding the glass rings by combining a cross stroke with rotation; thus translating, indeed, his geometrical conceptions into corresponding mechanism: and in realizing this design he found a zealous coadjutor in M. Soleil, by whom, with the encouragement of the French Government, the annular lens was successfully constructed. Fresnel's first lens was 30 inches square, and subtended at the focus 45°, vertically and horizontally; the focal distance being 36·22 inches (920 mm.). The lens now used in a first-order light, as shown in Fig. 2, has the same horizontal extent, but subtends 57° vertically, so that eight of them form a regular vertical prism, with a common focus, and enclose an equatorial belt of 57°, or about 47·7 per cent. of the whole luminous sphere, but in fact 55·75 per cent. of that portion of the sphere which the entire apparatus of glass embraces. The diagonal of the octagonal horizontal section is about two metres; which perhaps, therefore, was the origin of the present focal distance.

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[1] These considerations are not intended to apply beyond the actual ordinary limits within which refraction alone is employed in Lighthouse Apparatus.—J. T. C.

[2] This gradual diminution of the maximum deviation arises from a corresponding decrease of the angle subtended at the focus by the breadth of each successive ring as it is further from the axis; without which latter decrease the angles would project inconveniently, and the thickness of glass would become too great.—J. T. C.

There was still wanting a powerful flame; and for this purpose MM. Arago and Fresnel availed themselves of Count Rumford's idea of a multiple burner, and succeeded in constructing a lamp with four concentric cylindrical wicks. Carcel's contrivance for supplying and regulating an overflow of oil was essential to the due performance of the multiple burner; for unless it is cooled by a superabundance of oil, its accumulating heat not only volatilizes the oil, but also causes the deposit of carbon upon the wick. An adequate draught-pipe, with a contrivance for regulating its power, supplied a constant renewal of air for perfect ignition; and the proportionate quantities of air required for each individual flame, were secured by a corresponding ratio between the outer aperture and each of the inner ones by which air was admitted. This was determined by a series of experiments.

The intense heat of the four flames, which is rendered harmless by the overflow of oil, and by the rapid ingress of cool air, promotes such a thorough decomposition of the gaseous products of the oil, that a given quantity of it produces, in the four-wicked lamp, a greater illuminating effect than if burned in separate Argand, or Carcel lamps. Thus, if the French unit of light be adopted, which is that of a Carcel lamp 20 mm. in diameter, and burning 40 grammes of colza oil per hour, it is shown that a lamp with four wicks can be made to give the light of twenty-three such lamps, and yet will burn only 760 grammes of oil per hour, or what nineteen of the single lamps would consume.

It is remarkable how many inventors have contributed their respective parts to the multiple burner:—Argand, the double current; Lange, the indispensable contraction of the glass chimney; Carcel, the mechanism for an abundant supply of oil; and Count

Rumford, an idea, made feasible by these contrivances, and finally realized by Arago and Augustin Fresnel.

While the angle subtended by the flame at any point of the generating section of the annular lens decreases as the point recedes from the axis, the corresponding angle of divergence in the emerging beam does not decrease, but, on the contrary, it increases. Take into consideration, for example, the horizontal focal section in a first-order light. The angle subtended by the diameter of the flame at the lens varies from 5° 36′ at its centre, to 5° 12′ at its extremity; while between the same limits the corresponding angles of divergence, after transmission, vary from 5° 30′ to 5° 45′, in a converse progression.

The collective effect of the lens will be understood by what has been premised; it sends forward an infinite number of conical beams, which radiate from within its substance, and whose axes, as already defined, are all parallel to that of the lens; so that, at a moderate distance, the aggregate effect is one conical beam, whose axis is the lenticular one. The intensity of this collective conical beam varies in different directions, according to the corresponding parts of the flame from which the rays proceed; the maximum intensity is, of course, in the direction of the axis, from which the brilliancy gradually diminishes, until it becomes a minimum at the boundary of the beam. It has been found by observation that, in the horizontal plane, this gradation of intensity varies in a first-order lens from about 5,000 burners to 1,000 burners, of the French unit.

The refracting belt of the fixed light is cylindrical, and is formed by the revolution of the vertical central section of the annular lens round the vertical axis of the system, so that this belt is lenticular in every meridian plane, but not so in any horizontal one; and hence the central light retains its natural divergence in azimuth, and thus distributes, in every direction of the horizon, a uniform illumination.

The difficulty here, as with the annular lens, was the execution; and for years the refracting portion of the fixed light was a polygonal regular prism, the normal vertical section of each of its sides being the same as the meridian section of the cylindrical belt: but, of course, the illuminating effect in azimuth varied in each side, from its maximum at the middle vertical section, to its minimum at the angles. In the first-order light there were thirty-two sides.

The late Mr. Alan Stevenson, who had charge of the introduction into Scotland of the Fresnel system, was the first to carry out the cylindrical shape of the refractor: this he did at the Isle of May, where the first British dioptric fixed light was erected in 1836; the work having been executed at the manufactory of Messrs. Cookson and Co., of Newcastle, who subsequently constructed several lenses and cylindrical refractors for the Lighthouse Boards of this kingdom.

Mr. Alan Stevenson soon afterwards applied oblique joints to the cylindrical refractor, in order to avoid the intercepting of light caused by vertical ones.

The Commissioners of the Northern Lighthouses were the first to carry into effect, in this kingdom, the adoption of Fresnel's invention. It was proposed to them by their Engineer, the late Mr. Robert Stevenson, in consequence of a communication which he had received from General, then Major, Colby, R.E., at that time engaged in the Ordnance Survey of the British Channel.

And here it may be remarked, that the introduction of the dioptric system into this country had a zealous advocate in Sir David Brewster, who at once recognized its unquestionable superiority over the method of metallic parabolic reflectors.

The Catadioptric, or Totally-reflecting, Zones.

There is a limit[1] beyond which prismatic deflection becomes wasteful, partly by chromatic dispersion, and partly from the increasing loss by reflection at the surfaces of incidence and emergence. It occurred to Fresnel to employ totally-reflecting zones; and he actually introduced them above and below the refracting belt of his fixed Harbour Light, which was 30 centimetres in diameter; and it is asserted that reflecting segments, generated round a horizontal axis, were applied by him to a small apparatus at Paris, upon the Quay of the Canal St. Martin.

The late Mr. Alan Stevenson, however, the Engineer of the celebrated Skerryvore Lighthouse, was the first to extend the application of horizontal reflecting zones to dioptric apparatus of large dimensions. He introduced them in the lower portion of the revolving light which was placed at Skerryvore, and exhibited for the first time in February, 1843. They were executed by M. François Soleil, of Paris.

Mr. Thomas Stevenson, quite unaware of everything relating to the small instrument on the Canal St. Martin, proposed, on the 30th of March, 1849, in a Paper read before the Royal Scottish Society of Arts,[2] that reflecting prisms should be generated round a horizontal axis, so as to have a lenticular action, like that of the refracting lens. These prisms were first introduced by Messrs. Stevenson on the small scale at Horsburgh Lighthouse, near Singapore, which was shown to the mariner in October, 1851; and in January, 1851, the Commissioners of the Northern Lighthouses ordered vertical reflecting zones to be adopted in the first-order revolving apparatus intended for North Ronaldshay.

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[1] It is not assumed here that prismatic deflection is, at present, actually extended as far as it can be advantageously employed.—J. T. C.

[2] There seems to be no evidence that any account of the lenticular reflecting prisms of the Canal St. Martin Light was ever published, or that any proposal was made to employ such prisms for lighthouse purposes, previously to that of Mr. Thomas Stevenson on the 30th March, 1849.—J. T. C.

Hitherto, silvered mirrors—sometimes plane, sometimes concave,—had been used to show a fixed light beneath the great lenses of a revolving apparatus; and the rays above these lenses had been gathered into separate beams by small lenses, forming together a truncated pyramid above the flame, and then directed upon the horizon by a corresponding number of plane silvered mirrors. This arrangement was introduced at the first revolving light which was constructed under Fresnel's guidance, and which was exhibited at Cordouan in 1823: it is exactly the same in principle as that which Sir David Brewster devised for burning instruments, and which he described in 1812 in the Edinburgh Encyclopædia.

This invention of Sir D. Brewster is admirable for a burning instrument, because it intercepts a calorific beam of large diameter, and yet brings it to a minute focus; a result which a large lens cannot produce. But this very feature of the shortened focal distance unfits the plan generally for the purpose of condensing flame-light; and accordingly, in Fresnel's revolving apparatus, as the focal distance of the accessory lenses is less than one half of the shortest focal distance in the system of reflecting zones, the intensity of the light issuing from the former would be scarcely more[2] than one fourth of that transmitted by the latter; and, in addition to this cause of inferiority, is the loss arising at the mirrors; so that, on the whole, the modern plan must give light five or six times more intense than that of the former arrangement.

Of course Fresnel was well aware of these disadvantages; but he was limited to the contrivances which could in his time be executed. To compensate, however, in some measure for the reduction of intensity which arose from the short focal distance of the small accessory lenses, Fresnel obtained from them a flash of double horizontal divergence, and this he turned to good account, by causing it to precede that of the lenses, so as to increase threefold the duration of the total flash; the diminution of the length of eclipse being a point on which he laid great stress in his Mémoire, and on which the Engineers of the French Lighthouse Board still insist, as of more importance than the increase of the intensity of the flash.

The principle upon which Fresnel calculated the generating section of the reflecting zone, was that of dispensing with all superfluous glass.

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[1] These mirrors were also employed in fixed lights above and below the refracting portion of the apparatus.—J. T. C.

[2] The words, *scarcely more*, are used in order to allow for the greater loss of light caused by the prisms than by the lenses in consequence of the longer paths of the rays in glass.—J. T. C.

Let B F C, in Fig. 3, Plate 15, be an angle of light from the radiant point F; and B C A, the generating triangle, in the plane of B F C.

In order to avoid all redundant glass, the side C A must be the path of the ray F C, after its refraction at C, and the side B C must be the path of the ray F B, after its refraction and reflection at B. Hence, if C R be the direction of the ray B C, after emerging at C, the angles B C F and A C R are equal to each other; and the angle D C R, which the emerging ray makes with the incident one, being of course given, the angle B C F is determinable,[1] and therefore B C A.

The distance F C, and the angle B F C, are also given; so that the side B C of the section is known.

The reflecting side, B A, is curved; but instead of the true curve, a circular arc is necessarily adopted. The respective inclinations of this arc at B, and at its intersection with C A, are so determined that the refracted ray at B shall be reflected along B C, and that the ray C A shall be reflected in a path which, after refraction at the side C A, shall take the given direction at A.

The problem then is solved generally. In the particular case under consideration, the ray at A is made to emerge parallel to that at C; and in regard to the rest of the beam, so slight is the deviation that, for the ray which is incident at the middle point of B C, it is quite inappreciable: thus, in the first prism next to the refractor in Fig. 1, the deviation of this middle ray from a horizontal direction is only 3 minutes.

The slightest inaccuracy in the shape of the section will cause the emerging beam to be either diverging or converging, and, therefore, weakened in intensity in proportion to its increased dispersion in the plane of the section.

It will be evident that in all generating sections, for the same angle of light B F C, and the same condition of emergence, the angular elements will be constant; and that, if the length of F C is altered, the linear dimensions only will be changed.

The angles of incidence on entering the upper prisms decrease from 44° at the first of the prisms to 11½° at the furthest; and there is a similar diminishing progression from 27° to 7½° in the angles of incidence on emergence: but this may be considered to be compensated by the contrary order of progression in the angles

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[1] $\mathrm{BCA} = \frac{\pi}{2} + \sin^{-1}\left(\frac{\cos.\,\mathrm{BCF}}{\mu}\right)$, where μ = the refractive index: and $\mathrm{ACR} = \mathrm{BCF}$. Therefore $2\,\mathrm{BCF} + \frac{\pi}{2} + \sin^{1-}\left(\frac{\cos.\,\mathrm{BCF}}{\mu}\right) - \mathrm{DCR} = \pi$.

$$\therefore \cos \mathrm{BCF} = \mu \cos.\,(2\,\mathrm{BCF} - \mathrm{DCR});$$

or, if ξ = angle of incidence at C, and $\theta = \frac{\pi}{2} - \mathrm{DCR}$, $\sin \xi = \mu \sin (2\,\xi - \theta)$, as given by Mr. Alan Stevenson in his treatise.—J. T. C.

at which the light is incident, both on entering and on emerging from the glass chimney of the lamp.

It is suggested in the Appendix to the Report of the late Royal Commission, that the incidence on the prisms should be a normal one even at both the surfaces, external and internal. This can of course be done, it is merely to add superfluous glass, as shown in Fig. 4, and to calculate the reflecting side accordingly. But the consequent diminution of loss of light by reflection at the two surfaces would be far more than neutralized by the increased absorption resulting from the lengthened paths of the rays in glass, and also by the serious addition to the dimensions and weight of the apparatus, which latter effect even in a fixed light would be objectionable, and in a revolving one far more so.

It might be better in the reflecting section to cause the side B C to be the path of the ray which proceeds from the lowest part of the front of the corresponding section of the flame, because, in the present construction, a small portion of the prism at B is useless for all light below the focal direction. Also, strictly, each successive section ought to be so situated in the angle B F C, that the ray incident at B from the above-named lowest part of the flame should, on emerging at C, just graze the point A of the section next below it; at present the point C of the former and the point A of the latter are placed upon the same horizontal line.

In the smaller sizes of fixed lights no metallic rings are required between the refractor and each of the reflecting zones next above and below it; hence, in order to prevent the void spaces from subtending any angle at the focus, whereby light would be lost, the point C of the prism in each case must be outside the refractor, on the prolongation of the focal ray which touches its edge. In the employment of the electric spark in small apparatus this is absolutely necessary: and although it may be objected to this arrangement, that the extra size of each prism, unless an additional one be introduced, would cause increased loss by absorption, yet, when a flame is employed, this would be compensated for by the diminution in divergence corresponding to the lengthening of the focal distances.

The Method of Testing and Adjusting.

The paramount importance of extreme accuracy of shape and adjustment in every part of a dioptric apparatus has already been mentioned. It follows, therefore, that the essence of successful execution consists in the possession of a simple critical test of accuracy. Linear and angular measurements do not suffice. The most ready, and, at the same time, the most certain method of verification, is the optical one of internal observation; and the employment of this for the reflecting zones has produced a vast improvement in the efficiency of dioptric lights during late years. It is likewise applicable to the lens and the refracting belt; but as the method of conjugate foci, as explained by Mr. Alan Stevenson in his Treatise, could always be used in examining these portions, the plan of internal observation, although far more convenient and critical, was not so great a desideratum as in the case of the reflecting zones.

The system of internal observation during the process of manufacture is this: the ring or segment to be tested is fixed in a temporary frame in its due position relatively to the focus of the corresponding part of the apparatus, which point is indicated by a suitable instrument; a well-defined object is placed in front of the frame at a considerable distance, in the horizontal plane which bisects the part[1] of the glass piece under examination; the eye, placed at a convenient distance behind the focus, views the direction in which the image of the external object is seen through the middle of the section of the prism in the vertical plane passing through the focus and the object, and readily notices any deviation from the focus: also by moving the eye up and down in the vertical plane, it is easy to ascertain the position of the actual focus of the entire section for the pencil coming from the centre of the object, so as to determine whether the effect of the glass section is too converging, or the contrary.

The position of the due focus of the object will be very near to the focus of parallel rays, if the object is at a sufficient distance for that purpose. If the segment, or ring, be finally made to revolve round its axis of generation, every meridian section of it may be treated in like manner: but generally the simple motion of the eye, after a little practice, will, with proper allowance for the fixed position of the external object, suffice to extend the examination throughout the glass.[2]

The same process is adopted for the final adjustment and verification of the various parts of the apparatus in its permanent frame: the only difference being that the external object is placed in succession in the sea-horizon direction for each zone instead of in the horizontal line.

Similarly any dioptric apparatus may be adjusted and tested, however complicated the combination of its parts.

The method of adjusting by the image of the horizon began to be practised when the first Fresnel apparatus was erected at

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[1] In the case of a vertical ring or segment this part is, of course, a section made by the vertical plane through its axis of generation.—J. T. C.

[2] Strictly, the generating sections of the reflecting zones ought to agree with the due positions of their sea-horizon foci; and as an approximation, generally suitable, these sections might be made and tested in the first instance to correspond with a given depression of the horizon, such as an angle of ten minutes.—J. T. C.

Cordouan, but it seems to have been used only for the auxiliary mirrors. Its general application to lighthouse optical instruments is only of recent origin; and as the constructor and the inspector are thereby furnished with a test, which is perfectly accurate, and yet extremely simple, for examining both the shape and the adjustment of every part of an illuminating apparatus; and as the use of this system effected at once a thorough change in the practical part of lighthouse optics, it will not be a digression to allude briefly to the circumstances of its introduction.

The recent Royal Commission on Lighthouses deserves the merit[1] of first directing attention to the mode of examining sea-lights, by means of the sea-horizon image: and in doing so they availed themselves of the valuable scientific aid of the Astronomer-Royal.

In examining certain lights on the coasts of England and France, Professor Airy tested the adjustments and shapes of the various portions of each apparatus by observing with unaided vision in what directions the axes of pencils of light from the horizon, or from objects on the sea, crossed the burner of the lamp; and also where on their respective axes the foci of these pencils were situated.

He was much struck with the extreme importance, as well as with the simplicity, somewhat unexpected, of a test so searching and infallible. After his visit to the lights at Whitby he wrote to the Author in June, 1860, thus:—

"I very much wish that I could induce you to look at the Whitby lights. I think that it would lead to an extensive and beneficial revolution in lighthouses."

The Author soon afterwards, at the request of the Trinity Board, undertook the readjustment of the Whitby lights, and was at once satisfied, that the internal method of observation fully realized the value attached to it by the Royal Commission and by Professor Airy.

Fortunately the weather was hazy during several days, which rendered it necessary to resort to some substitute for the sea-horizon: a staff, Fig. 1, was fixed vertically upon an elevated position in the vicinity, and a middle belt of one of the refractors being used as a theodolite, the level was taken of its centre: the depression due to the dip of the visible horizon was then allowed for, and the staff was graduated so as to correspond with the successive zones above and below the refracting portion. The apparatus itself was made to revolve, in order to place every segmental division in its turn opposite the staff, without disturbing the level of the focal plane.

The adjustments were then effected; and when the atmosphere became clear, they were found to correspond exactly with those which the images of the sea-horizon itself would have indicated.

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[1] The Commissioners in their Report attribute to their Secretary, Mr. J. F. Campbell, of Islay, the valuable plan of internal observation.—J .T. C.

It was, therefore, evident that, by pursuing a similar process at the manufactory, the most unerring certainty of final accuracy of adjustment might be insured. This the Author had an opportunity of at once putting into practice; inasmuch as the three dioptric lights that were destined for the iron towers, which Mr. W. Parkes, M. Inst. C.E., had designed for lighthouses in the Red Sea,[1] were waiting for their final adjustment. The result considerably exceeded that which was anticipated: not only was perfect accuracy attained, but the operation of adjustment was rendered far more rapid than what could previously be accomplished.

One rule, however, is imperative: it will be evident that not a segment of glass should be placed in an apparatus before the whole framework has been fitted together, just as it will be at its ultimate destination, and has been accurately levelled.

Nothing could be more unscientific than the system which was, until a recent date, frequently practised by the lighthouse authorities of this country: the manufacturer of lighthouse apparatus often supplied only the separate panels with the glass permanently fixed in them; and an intervening constructor was employed to frame them together.

There are many serious objections to such a course. First, it is almost impracticable to secure accuracy in the first instance, if in adjusting the glass the apparatus is treated in successive portions and not as a whole: Secondly, the primary adjustments, however carefully they may have been made, will invariably be altered in the hands of the second person; for an error of even the one hundredth of an inch in the level of any part will cause a serious deflection: Thirdly, the responsibility is divided.

Perhaps it is scarcely necessary to add, that during the adjustment of the glass zones the frame of the apparatus should not be disturbed. Thus if a workman supports himself on the frame, the level may be deranged during the process: and also in the case of a revolving light, any horizontal oscillation of the apparatus should be securely prevented.

THE PARABOLIC METALLIC REFLECTOR.

This instrument is still employed in one half of the sea-lights of this kingdom. In January, 1867, there were the following lights on the coasts of the United Kingdom:—

	Dioptric.	Catoptric.	Total.
England and the Channel Islands . .	35	38	73
Scotland and the Isle of Man . . .	31	20	51
Ireland	25	30	55
	91	88	179

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxiii., p. 1, *et seq.*

The parabolic mirror must not, therefore, be passed by unnoticed.

The idea of its application to sea-lights soon followed the invention, in 1784, of the cylindrical burner with its double current of air. The chimney, that was essential to perfect combustion, served likewise the indispensable purpose of carrying off the gaseous products, which in previous forms of lamp, by tarnishing the surface of a reflector, rendered its adoption quite futile.

Argand, who is generally recognized as the author of this valuable lamp, seems to have perceived at the same time the applicability of the parabolic reflector for sea-lights; and Teulère, who, as early as 1783, proposed the latter arrangement, has also some claim to have originated, independently of Argand, the idea of the double current burner.

Teulère's reflector was carried into effect by Borda at the Lighthouse of Cordouan, and it is remarkable that on this tower were exhibited the first sea-light consisting of parabolic mirrors, and, about thirty years later, the first Fresnel dioptric apparatus.

It should not be omitted that parabolic reflectors, composed of facets of silvered glass fixed in a plaster mould, were erected in 1787 at Kinnairdhead, in Aberdeenshire, under the direction of the Northern Lights' Board; being the contrivance of their Engineer, Mr. Thomas Smith, who seems to have been quite ignorant of what was being suggested in France with the same object.

Sir David Brewster,[1] and other eminent writers on light, have shown how much greater is the loss of power when rays are reflected from a metallic surface, especially if hammered into shape, as in the case of the ordinary parabolic reflectors, than when transmitted through glass lenses or prisms of moderate thickness. Experimental results to the same effect are given by Mr. Thomas Stevenson in his work on Lighthouse Illumination, published in 1859; and he also points out the great superiority of glass in comparison with the metal of lighthouse reflectors, in admitting and retaining a high polish and accuracy of shape.

But, apart from these considerations, the lighthouse reflector gives place to the dioptric instrument for two other reasons mainly. First, the parabolic mirror irremediably causes great waste of light, and therefore of oil, by useless divergence: Secondly, it is only by an enormous multiplication of reflectors, far beyond what, in the presence of a better system, engineering principles would justify, that the power of dioptric sea-lights can be rivalled. Theory and experiment concur in this result.

There are three principal sizes of parabolic reflectors which are adopted in this country. The English type of mirror has an aperture of 21 inches and a depth of 9 inches, which give a focal distance of 3 inches at the vertex. The ordinary Scotch reflector has the same aperture, but the focal distance of its vertex is 4 inches, which give a depth of nearly 7 inches; but in Scotch revolving lights another size of reflector is also used, which has the same focal distance at the vertex, but an aperture of 25 inches, and therefore a depth of rather more than $9\frac{3}{4}$ inches.

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[1] *Vide* Transactions Royal Society of Edinburgh, vol. xi., 1831.

Were it not for the shadow of the burner, and the small aperture occupied by the chimney, the following would be the portions of the luminous sphere included by the English, the Scotch Fixed, and the Scotch Revolving types respectively, namely,

English.	Scotch Fixed.	Scotch Revolving.
74·6	63·3	71 per cent.

The burner used in England has a diameter of $\frac{7}{8}$ths of an inch; that in Scotland has a diameter of one inch.

The theoretical angles of divergence at the vertex, at the extremity of the parameter, and at the terminating point of the horizontal generating parabola, are:—

	Vertex.			Extremity of Parameter.			Edge.		
	°	′	″	°	′	″	°	′	″
In the English Reflector	16	25	36	8	11	32	4	9	26
In the Scotch Ordinary Reflector .	14	21	40	7	10	0	5	15	46
In the Scotch Revolving Reflector.	ditto.			ditto.			4	9	48

Mr. Thomas Stevenson places a lenticular front upon the parabolic mirror, Fig. 5, so as to condense the cone of light which would otherwise pass off in its natural state of divergence; and in place of the corresponding back portion of the reflector, he substitutes a spherical metallic mirror, which returns the flame upon itself, though inverted.

Let it be assumed that, with this modification of Mr. Stevenson, the proportion of the luminous sphere, which the parabolic mirror and its adjuncts condense, is equal to that which is embraced by a complete dioptric instrument; still the defect of wasteful divergence remains.

There is a practical limit to the dimensions of the reflector; and perhaps it would be found inexpedient to extend the size beyond that of the Scotch instrument, whose aperture has a diameter of 25 inches.

If, again, with a given maximum size of reflector the diameter of the burner be enlarged without the introduction of a further wick, there will be a corresponding increment in the divergence of the beam, but very little, if indeed any, addition to its mean intensity.

There may be a slight increase in the intensity of the flame itself, arising from the more active combustion which accompanies increased heat; but this advantage will be small in amount. And even if a further wick be introduced, the proportionate increment of mean intensity will be much below that of the consumption of oil.

In order, therefore, to obtain an intensity of illuminating power at all approaching that of a dioptric instrument of the higher orders, there is no resource but to multiply the number of the separate reflectors.

For the purpose of estimating the exact multiplication of reflectors which would be required, recourse must be had to experiment; but unfortunately in this kingdom there is no national institution corresponding to the 'Etablissement Central des Phares' at Paris; and hence for experimental statistics in this matter the results obtained in France must be consulted. M. Léonor Fresnel, in his communication dated the 31st December, 1845, to the Lighthouse Board of the United States, drew up an elaborate account of the comparative advantages of the system of metallic reflectors and dioptric instruments for sea-lights. Those results, however, require to be revised, in consequence of the improvements which have been effected in the Fresnel system since that date; and, accordingly, more reliable figures of comparison may be met with in later publications emanating from the French Lighthouse Engineers. In the Mémoire of M. Reynaud will be found a very complete comparison, based upon actual photometrical observations, of the relative economical and useful merits of the two rival systems of metallic reflectors and dioptric instruments.

M. Reynaud shows that a Fresnel light of the fixed kind, even of the second order, can be equalled by reflectors only by multiplying them to the number of 60, each giving about the same quantity of light in the horizontal plane as the English[1] reflector; and that the consumption of oil will be seven times more in the employment of these reflectors than in the case of the dioptric apparatus. In England, a fixed light of reflectors has them generally in the proportion of 24 to 27 in number for 360°. A first-order fixed Fresnel light gives nearly double[2] the intensity of that of a second-order one; and, accordingly, to rival this apparatus, the number of the reflectors must be about 108: but this is, of course, purely an imaginary structure. Yet, even with all this multiplying of reflectors, the perfection of uniformity in the distribution of light over the horizon, which accompanies the fixed dioptric light, cannot be imitated by parabolic mirrors.

From the foregoing estimate of the number of reflectors required for an apparatus which would be equal in power and general effect to a first-order dioptric fixed light, it may be calculated approximately what arrangement of reflectors would be necessary, in order to produce the effect of a first-order dioptric revolving apparatus.

Let it be supposed, for example, that this light has eight sides, and that the axes of the upper, middle, and lowest panels respectively, have slightly different directions in azimuth, so that the horizontal divergence shall be one-half of that of the reflectors; then, the catoptric apparatus should consist of four sides, each of which should carry twenty-seven reflectors of the English size.

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[1] Allowance being made for the larger diameter of the French burner employed in these experiments, as compared with that of the English one.—J. T. C.

[2] The ratio is that of 630 to 335 according to the French experiments.—J. T. C.

One point of advantage in the dioptric apparatus should not be forgotten. No one can visit a light consisting of reflectors without finding some of them out of adjustment, in relation to the position of the burner, or the direction of the axis of the paraboloid. Indeed, in a revolving light, it is a matter of no little nicety to place, and to keep permanently in due parallelism, all the axes of the reflectors which have to co-operate together on the same face of the frame. Whereas, in a dioptric light, the optical apparatus itself is adjusted irremovably, once for all; and the only deviation which can take place in the position of the burner is on the occasion of changing it; but the provision for indicating its due adjustment in every respect is so simple and unalterable, that nothing but the most wilful neglect can produce any error.

CATADIOPTRIC, OR TOTALLY-REFLECTING, SPHERICAL MIRROR.

Until late years, the metallic spherical reflector was the only resource for returning the back hemisphere of rays, or a portion of it, upon its luminous source. Just, however, as the metallic surface of the paraboloid has been condemned, that of the spherical reflector is similarly objectionable. But it has another serious defect: the reflected flame has an inverted position; so that either the chief portion of the reflected rays must fall upon the burner, or else the focus of the reflector must be raised so far above the burner, that the main reflected light, when transmitted by the dioptric instrument in front, falls far within the sea-horizon direction: the latter alternative, however, although not satisfactory generally, should be adopted.

And here it is well to remark, that many metallic reflectors, now useless in some British lighthouses, might be made available, as far as their limited capabilities extend, by readjusting the focus in relation to the burner.

Fortunately, however, the metallic spherical reflector has been superseded, for sea-lights, by the catadioptric one, which was originated by Mr. Thomas Stevenson, and may be thus described.

Figure 6 represents the sections which, by revolving round the axis of the flame, generate the totally-reflecting mirror, and shows to scale the instrument which is used in the larger sea-lights; the dimensions being reduced for the smaller apparatus.

The inner surfaces are zones of spheres which have a common centre, F, in the axis of the flame, at the centre of its effective portion. They constitute a perfect spherical mirror for that faint amount of light which is superficially reflected. The characteristic feature, however, of the instrument is that which concerns the main portion of incident rays which enters these inner surfaces.

Let the two outer sides of any generating section be supposed to be parabolic arcs, A B and A C (Fig. 7), having a common parameter, A F: a ray, F P incident at P, beyond the critical angle, is totally reflected in a path which is perpendicular to the parameter, and meeting the other arc at Q, is again totally reflected in the direction Q F. The parametral ray F A is reflected along A F. By the property of the parabola, the angle of incidence of F A at A is 45°, and that of F P at P is $\left(45^\circ - \frac{AFP}{2}\right)$. Hence at either extremity, as at B, $\left(45^\circ - \frac{AFB}{2}\right)$ must not be less than $\sin^{-1}\frac{1}{\mu}$, where μ is the refractive index of the least refrangible ray of the spectrum. This condition determines the maximum value of A F B, supposing the radiant body to be a point. Consider, however, the angle F B H subtended by the flame on the side of F B, where the normal at B is situated: the angle of internal incidence of H B at B is $\left(45^\circ - \frac{AFB}{2} - \sin^{-1}\frac{\sin FBH}{\mu}\right)$, and this angle must not be less than $\sin^{-1}\frac{1}{\mu}$ from which condition the maximum value of A F B, corresponding to F B H, is obtained.

Similarly, the maximum value of A F C can be found: but the limit of B F C is taken as twice the lesser angle, otherwise the section would not be symmetrical.

In the actual execution of the zone, each of the arcs A B, A C, is circular: the radius at A coincides with the normal at that point to the parabolic arc, and the radius at the extremity is parallel to the normal to the parabolic arc at its extremity. Therefore the angular positions of these two radii are known; and hence the coordinates of the centre of curvature and the radius are determined.

The image of the flame will coincide with the original, except that it will be simply turned half round the vertical axis.

A full mathematical investigation, by Professor Swan, will be found in the Appendix to the Treatise of Mr. Thomas Stevenson. But it will be perceived that the zones are supposed to be generated round a horizontal axis. The image will alternately pass from its erect position to an inverted one, and conversely through the successive quadrants, beginning at the highest or lowest points of the mirror.

The vertical arrangement of the zones not only presents difficulties of execution, but also does not permit the mirror to be so readily restricted within any desired limits in altitude, as if they are horizontal.

The plan of generating the zones round the vertical axis was introduced by the Author, who adopted it in the first complete catadioptric mirror which was made, and which was shown in the Exhibition of 1862 by the Commissioners of Northern Lights, for whom it was constructed, in order to further the realizing of what Mr. Thomas Stevenson had ingeniously suggested about twelve years previously.

During the progress of this instrument, the idea occurred to the Author of separating the zones, and also of dividing them into segments, like the ordinary reflecting zones of a dioptric light: by this means it became practicable to increase considerably the radius of the mirror, and thereby to render it applicable to the largest sea-light, without overstepping the limits of the angular breadths of the zones, and yet without being compelled to resort to glass of high refractive power.

The separation of the zones also rendered it feasible to avoid giving to the aggregate structure a spherical shape, which would have encroached most inconveniently upon the space required for the service of the lamp.

This improvement was carried into effect towards the end of 1862; and early in 1863 two mirrors were constructed for Messrs. Stevenson, as accessories to two fixed sea-lights intended for the coast of Otago, New Zealand: one being a first-order apparatus for Cape Saunders, the other a third-order light for Tairoas Head.

The same types have been retained unchanged to the present time, and have been used extensively both in fixed and in revolving lights.

Mr. Thomas Stevenson's Azimuthal Condensing System.

A valuable feature in the dioptric apparatus is its ready adaptability to special requirements. Take the case in which a fixed light, of a given power, has to illuminate a portion only of the azimuthal circle, but where in one or more directions greater intensity is wanted. Mr. Thomas Stevenson solved a problem of this kind at Isle Oronsay in October, 1857. Rather less than a semicircle had to be lighted; but two small portions of the illuminated sector, one on either side, required a power much exceeding that of the rest of it. The landward residue of the 360° was accordingly divided into two suitable parts, each of which was made to transmit its light in a series of angles parallel to the corresponding angles whose illumination required to be intensified. Without this arrangement a number of separate reflectors and lamps must have been used for the purpose. A full account of this light will be found in Mr. Thomas Stevenson's Treatise, already alluded to. The horizontal deflection in a case of this kind is effected by vertical reflecting or refracting prisms. The apparatus at Oronsay was one of the smaller order.[1] The Author, however, applied a similar method to a first-order apparatus at Great Orme's Head, in 1862, for the Mersey Docks and Harbour Board, and subsequently at Gibraltar for the Trinity Board, in each of which lights there was a spare arc, and increased power was required in a particular sector of the sea-surface for the purpose of strengthening a red beam. In each case a group of vertical prisms is fixed outside the spare arc, whose light is thus utilized, consisting of three tiers, which correspond respectively to the refracting and the two reflecting divisions of the instrument, and having, in all, a height of about 9 feet. The design for Gibraltar (Fig. 8) demanded more contrivance than that for Great Orme's Head. One chief point was, to avoid excessive obliquity of incidence on the lantern panes: for this, and other reasons, the reflecting prisms, R, were made to act together as a single cylindrical concave mirror, which brought the rays into an approximate focus, from which they diverged in the required directions. This concave grouping of the vertical deflectors, provided a most convenient space for the introduction of a single parallelizing vertical prism, P, which would send a strong beam along the intended boundary of the red arc. A screen of red glass, S, was situated between the main apparatus and the accessory upright prisms. As each tier of prisms would, if fixed in their frames, be liable to accident while being transferred and erected in their places; but as, on the other hand, it was absolutely essential that the final adjustment of these vertical prisms should be an accurate imitation of what had been originally performed in the first construction, every vertical prism was transported apart from its frame: but, previously to its removal, brass templates were fitted with the greatest exactitude, to indicate the precise due position of each prism. What was finally carried into effect at the destination of the apparatus was, accordingly, an exact reproduction of what had been done at the manufactory, with the nautical chart as a guide.

From these examples it will be evident, that subsidiary parabolic reflectors are not required generally for the purpose of intensifying the light in particular arcs. On the contrary, reflectors are objectionable, inasmuch as they are not suitable for defining sharply the due confines of an arc. For even if the natural radiation in front of the reflector be condensed, as by Mr. Thomas Stevenson's anterior lens, yet, since the divergence of the reflected light increases from the edge of the mirror towards its vertex, or to the centre of the front lens, the inner conical beams cross the outer ones, and produce a penumbral light, increasing in faintness outwards, which is spread over a large angle on either side of the arc requiring illumination, and which it is generally inconvenient to intercept effectively, if indeed practicable.

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[1] The Author designed an apparatus for Dartmouth Harbour (Fig. 10), for Mr. R. P. Brereton, M. Inst. C.E., in which two arcs of red and green light respectively were strongly intensified by vertical reflecting prisms.—J. T. C.

Hence this system of illuminating particular arcs is in every respect advantageous. It need scarcely be added, as a mere corollary of what precedes, that for leading lights the dioptric azimuthal system is peculiarly suitable. The Author some years ago designed two for Hoylake on this principle, for the Mersey Docks and Harbour Board; and he has lately constructed two according to Mr. Thomas Stevenson's design for Buddonness, at the entrance of the Frith of Tay.

In both cases a fixed apparatus of 180° of the ordinary kind is employed; and vertical prisms, which deflect horizontally, are placed in the complement of each half of the illuminated angle, and distribute over it equably their respective diverging beams.

To the Buddonness apparatus (Fig. 9), however, Mr. Stevenson has added some ingenious arrangements, by which the chief portion of the back hemisphere is sent forward, and uniformly spread over the illuminated sea-sector. The equatorial belt of about 60°, or one-half of the back light, is returned upon itself by the totally-reflecting mirrors already described; but the novelty consists in dealing with the half cone of light which diverges above this mirror. It is first condensed cylindrically by a compound semi-lens, and then deflected horizontally, as well as uniformly expanded over the illuminated direct arc, by means of a series of right-angled prisms, in circular segments, placed above the rest of the apparatus. The curvature of these segments, which should be convex outwards, ought to increase from the foremost in succession backwards, in proportion to the diminution of the section of the vertical beam which each acts upon.

The spherical mirror is made to open by hinges, in order to give access to the interior of the apparatus.

The fixed light has a diameter of 29½ inches; and the height of the apparatus, exclusive of the upper reflectors, is 4 feet.

A full-sized model of this instrument is now at the Paris Exhibition. It is especially interesting, as combining every existing dioptric method employed in lighthouses.

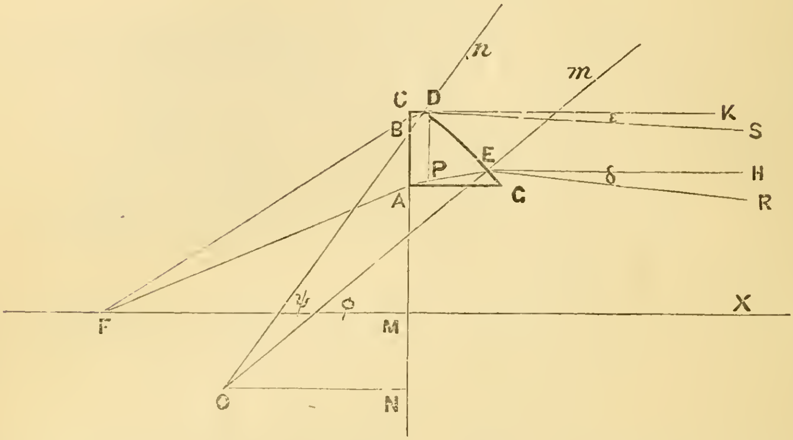
In the Appendix will be found the mathematical investigation of the various problems referred to in this communication, which is also accompanied by a series of diagrams from which Plate 15, and woodcuts, Figs. 1 to 5, have been compiled.

[APPENDIX.

APPENDIX.

Generating Section of a Refracting Zone, or Straight Prism.

Fig. 1.



A C D G is the section: F a radiant point. F X is perpendicular to the side C A produced; the section being plano-convex.

Let F A E R, F B D S, be the extreme rays, making with F X the angles δ, ϵ, respectively, on emerging: D K and E H being parallel to F X.

O E m, O D n, are normals at E and D, to the convex side D G, which is circular, so that O E, O D, are radii of it: let them make with F X the respective angles ϕ and ψ.

Let α and ρ be the angles of incidence and refraction at A; β and σ those at B:

$$FM = f,\ CD = t,\ AC = b = f\,.\,\overline{\tan\beta - \tan\alpha} + t\,.\,\tan\sigma.$$

Then,
$$\sin\rho = \frac{\sin\alpha}{\mu},\ \sin\sigma = \frac{\sin\beta}{\mu}.$$

The angle of incidence at E $= \phi - \rho$

,, ,, D $= \psi - \sigma$;

$$\sin(\phi + \delta) = \mu\sin(\phi - \rho) \text{ and } \sin(\psi + \epsilon) = \mu\sin(\psi - \sigma).$$

Therefore,
$$\tan(\phi + \delta) = \frac{\mu\sin(\rho + \delta)}{\mu\cos(\rho + \delta) - 1},$$
$$\tan(\psi + \epsilon) = \frac{\mu\sin(\sigma + \epsilon)}{\mu\cos(\sigma + \epsilon) - 1};$$

whence ϕ and ψ are determined.

Draw D P parallel to C A intersecting A E at P, then in the triangle D E P, the

$$\text{chord } DE = DP\,.\,\frac{\cos\rho}{\cos\left(\frac{\phi + \psi}{2} - \rho\right)};$$

where
$$DP = b - t\,.\,\tan\rho.$$

Therefore, if r = radius of curvature of the arc D G,

$$r = \frac{DE}{2}\,.\,\frac{1}{\sin\frac{\psi - \phi}{2}},$$
$$= \frac{(b - t\,.\,\tan\rho)\,.\,\cos\rho}{2\sin\frac{\psi - \phi}{2}\cos\left(\frac{\psi + \phi}{2} - \rho\right)};$$

and, for the co-ordinates of O, referred to M as the origin,

$$ON = r\cos\psi - t, \text{ and } NM = r\sin\psi - f\tan\beta - t\,.\,\tan\sigma.$$

OPTICAL APPARATUS OF LIGHTHOUSES.

Fig 1

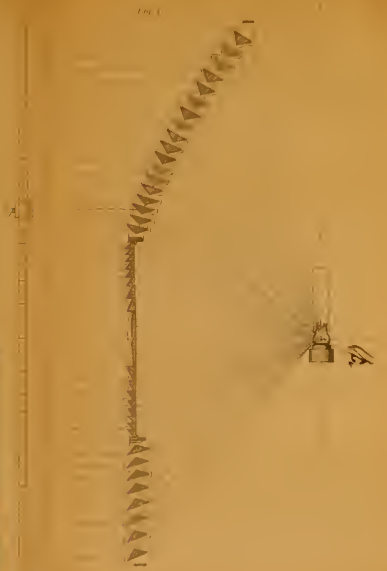


Fig 2

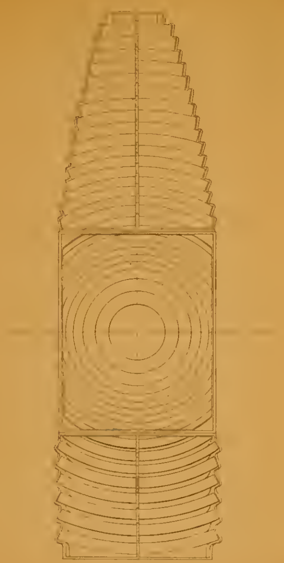


Fig 3

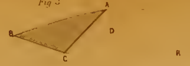


Fig 4

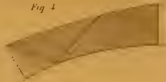


Fig 5

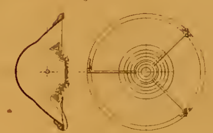


Fig 6

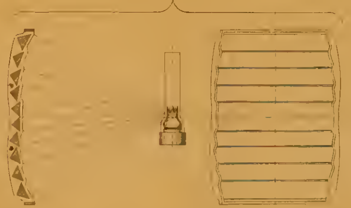


Fig 7

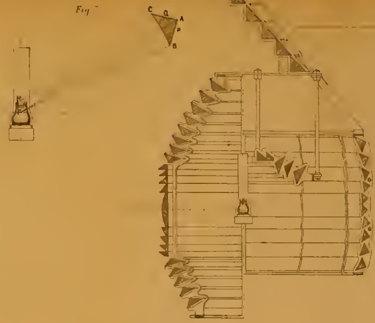


Fig 9

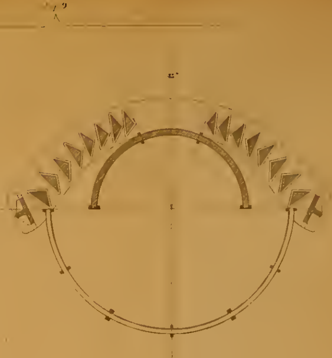


Fig 8

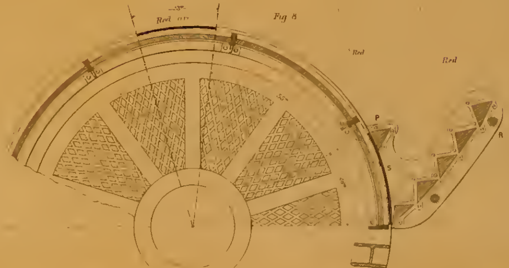
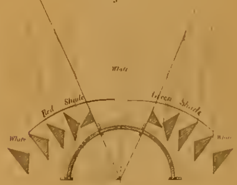


Fig 10



Scale

I. If the section be required for a prism, which is detached, $t = o$ generally.

II. If the emerging rays be parallel to each other, $\delta = \epsilon$; and if they are parallel to the axis F X, $\delta = o$ and $\epsilon = o$, as in the ordinary section of Fresnel.

III. If either emerging ray pass between the normal at the point of emergence and the axial direction, the corresponding angle δ, or ϵ, will be negative.

IV. If F A be perpendicular to C A, $\alpha = o$, $\rho = o$.

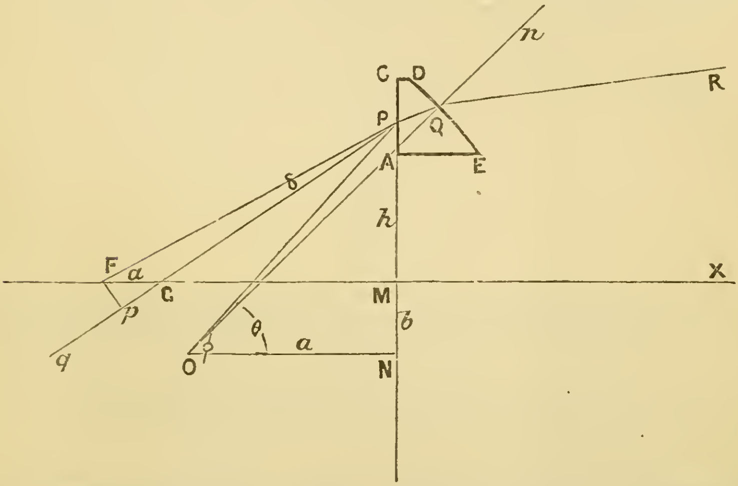
V. If the joints of the zones are inclined, in the directions of the refracted rays, the foregoing formulæ will remain the same; the angles of glass E A G and C D B being removed, so that the actual section will become A B D E.

VI. If C A be a circular arc, either concave or convex, the angles of incidence will be changed accordingly; again, the side of emergence may be made concave instead of convex, in which case $\psi - \phi$ becomes negative, and $\dot{r}$ is negative; but the plano-convex form is that which circumstances most generally require.

VII. By commencing from the point C or the point B in the same way as that adopted in the foregoing problem, the sections of the successive zones may be similarly calculated for the Fresnel lens or cylindrical refractor.

To Determine the Path of any Ray.

Fig. 2.



Let A C D E be a generating section, as determined by the preceding problem for the extreme rays from a given radiant point F.

Let any ray q P, crossing the axis at G, be incident upon a point P of the lens, and describe the path P Q R.

Draw F p perpendicular to q P: join O P, and O Q which produce to n : n Q R is the angle of emergence.

Let $\quad ON = a, NM = b, MP = h, OQ = r,$
$\quad PFM = \alpha, FPG = \delta, FM = f, Fp = d,$
$\quad PON = \theta, QON = \phi,$
the angle of refraction of q P at P $= \rho$,
the angle of emergence of P Q at Q $= \eta$.

Then $\sin\delta = \dfrac{d}{f} \cdot \cos\alpha$, and $\sin\rho = \dfrac{\sin(\alpha + \delta)}{\mu}$, $\tan\theta = \dfrac{b + h}{a}$,

and in the triangle P O Q, $\sin(\phi - \rho) = \dfrac{OP}{OQ} \sin(\theta - \rho)$,

$$= \frac{a}{r} \cdot \frac{\sin(\theta - \rho)}{\cos\theta} \qquad (1);$$

$$\text{also, } \sin\eta = \mu \sin(\phi - \rho) \qquad (2);$$

whence ϕ and η are determinable; and $(\phi - \eta)$, which is the angle made by the emerging ray Q R with the axis F X.

I. If the incident ray cross the axis beyond F, δ is negative.

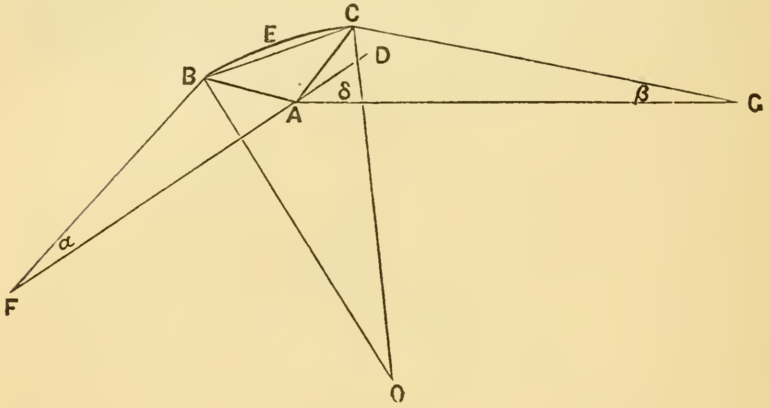
II. If $\delta = o$, we have the paths of the *focal* rays at the successive points of the lens.

III. If $(\phi - \eta)$ be negative, the ray Q R crosses the axis F X on the outer side of the refracting section.

IV. From the triangle P O Q is obtained the length of the path P Q for any ray in its passage through the glass.

TOTALLY-REFLECTING PRISM.

Fig. 3.



A B C is the generating section of a totally reflecting prism, upon which is incident in the plane of the section the angle of light A F B from the radiant point F.

Let A G and C G be the directions of the extreme emerging rays.

Let A F B = α, A G C = β, the angle of incidence of F A at A = θ; produce F A to D and let D A G = δ.

In order to avoid superfluous glass, the sides A B and A C are made to coincide with the paths of the rays F B and F A: hence the angles B A F and C A G are equal to each other; and

$$\text{B A C} = \frac{\pi}{2} + \sin^{-1}\left(\frac{\sin\theta}{\mu}\right).$$

Therefore, $$2\left(\frac{\pi}{2} - \theta\right) + \frac{\pi}{2} + \sin^{-1}\left(\frac{\sin\theta}{\mu}\right) = \pi + \delta,$$

$$\sin\theta = \mu \sin\left(2\theta + \delta - \frac{\pi}{2}\right),$$

from which equation θ can be found tentatively.

Let ρ, ϕ, ψ be the angles of refraction at A, B, and of internal incidence of the emerging ray C G, respectively.

$$\text{Sin}\ \rho = \frac{\sin\theta}{u},\ \sin\phi = \frac{\sin(\theta - \alpha)}{\mu},\ \sin\psi = \frac{\sin(\theta - \beta)}{\mu}.$$

Draw at B and C the radii B O, C O, of the circular arc B E C which is the reflecting boundary of the prism; and draw the straight line B C.

$$\text{A B O} = \frac{1}{2}\left(\frac{\pi}{2} + \phi\right),\ \text{A C O} = \frac{1}{2}\left(\frac{\pi}{2} + \psi\right).$$

The angle $B O C = B A C - (A B O + A C O)$.

Therefore, $$B O C = \rho - \frac{\phi + \psi}{2},$$

and as $B C$ is circular, $O B C = O C B = \frac{\pi}{2} - \frac{1}{2}\left(\rho - \frac{\phi + \psi}{2}\right)$.

Therefore, $$A B C = O B C - A B O = \frac{\pi}{4} + \frac{\psi}{4} - \frac{\rho}{2} - \frac{\phi}{4},$$

$$A C B = O C B - A C O = \frac{\pi}{4} + \frac{\phi}{4} - \frac{\rho}{2} - \frac{\psi}{4}.$$

Let $F A = f$, then $A B = f \, . \, \frac{\sin \alpha}{\cos (\theta - \alpha)}$, $A C = A B \, . \, \frac{\sin A B C}{\sin A C B}$,

chord $B C = A B \, . \, \frac{\sin B A C}{\sin A C B}$, and radius of curvature $= \frac{B C}{2} \, . \, \frac{1}{\sin \left(\frac{B O C}{2}\right)}$.

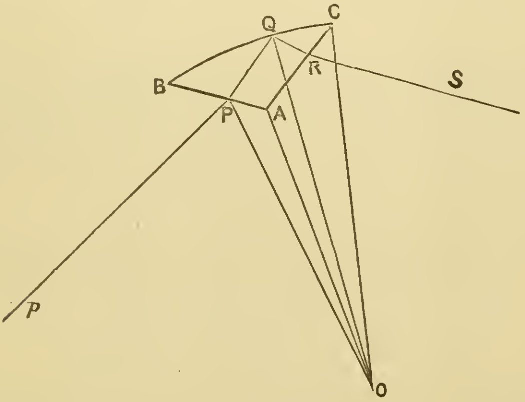
If the emerging rays be parallel, $\beta = o$, and $\psi = \rho$.

If the emerging rays be diverging, β is negative.

In order to facilitate the construction of the prism, the points B and C, and the centre of curvature of B E C, are referred to axes of co-ordinates, which may be chosen as may be most convenient in practice.

To Determine the Path of any Ray.

Fig. 4.



Let $pPQRS$ be any ray.

O is the centre of curvature of the reflecting side B C.

Join O P, O A, O Q, O C.

In the triangle A C O, the two sides A C, C O, and the included angle at C, are known:

hence from the equations, $\tan \frac{1}{2}(C A O - A O C) = \frac{C O - C A}{C O + C A} \cot \frac{A C O}{2}$,

and $$C A O + A O C = \pi - A C O,$$

are determined C A O, and A O C: hence A O is obtained.

Again, in the triangle A P O, P A is given, A O has been determined, and $P A O = 2\pi - (B A C + C A O)$; hence, as in the previous case, A P O and P O are found.

Now as the direction of p P is given, the angle Q P A is known; hence in the triangle P Q O we have P Q O from the equation

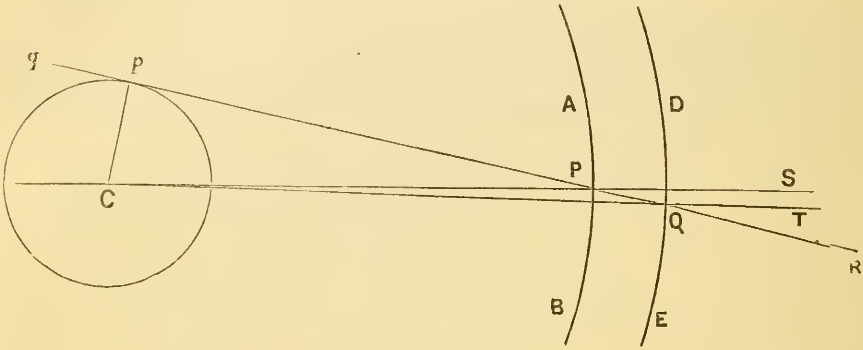
$$\sin PQO = \frac{OP}{OQ} \cdot \sin QPO,$$

and
$$PQR = 2\, PQO,$$
$$QRA = 2\pi - (QPA + BAC + PQR),$$
and
$$\cos CRS = \mu \cos QRA;$$
whence is obtained the direction of R S the emerging ray.

The length of the path P Q, Q R, of the ray through the prism is also obtained.

THE APPARENT DIAMETER OF THE FLAME IN THE FOCAL PLANE OF A FIXED APPARATUS IS NOT CHANGED BY THE INTERPOSITION OF THE REFRACTING ZONE.

Fig. 5.



A B E D is a segment of a horizontal section of a lenticular zone generated round a vertical axis through C. Let a ray of light q P in this section be incident at P and take the path P Q R. Join C P and C Q; draw C p perpendicular to q P; and produce C P and C Q to S and T respectively.

In the triangle P C Q, $\sin PQC = \frac{CP}{CQ} \sin QPS$;

therefore,
$$\sin TQR = \mu \sin PQC = \frac{CP}{CQ} \mu \sin QPS$$
$$= \frac{CP \sin qPC}{CQ} = \frac{Cp}{CQ};$$

hence if R Q the emerging ray be produced, it will touch the circle described round C with the radius C p.

Mr. Gregory, Vice-President, said, the applause of the meeting had already anticipated the cordial vote of thanks which he was sure every one would accord to Mr. Chance for this valuable Paper—a Paper which would be a most useful addition to the archives of the Institution, and would derive additional interest from the fact of the Author having brought his attainments in exact science to bear upon his well-known practical knowledge as a manufacturer. As a juror at the Paris Exhibition, his attention had been called to some of the results of Mr. Chance's labours, to which the Paper had so modestly referred. The apparatus exhibited by the Trinity House and by the Commissioners of Northern Lights, to the excellence of which Mr. Chance had materially contributed, excited the admiration of his brother jurors, and particularly of M. Reynaud, the distinguished Engineer at the head of the Lighthouse Department of France, and were felt by all to do credit to this country.

Mr. Chance said, it was only right he should mention, that the idea of presenting a Paper to this Institution upon a subject of so much importance did not originate with himself. It had, however, afforded him the greatest pleasure, which was enhanced by finding that the communication was regarded as in any way useful to the Institution. By way of explanation, it was important to add, what no doubt had been already perceived, that the purely optical part only of the subject had been dealt with; and that the Paper did not pretend to give a complete view of this branch of engineering. There were several mechanical considerations in the structure of the apparatus, in order to give the optical part the greatest possible effect, and also for producing in the best manner the movements required, whether in the rotation of the main apparatus or in providing the overflow of oil, to which allusion had been made. He had not treated of these, and if any one would do so, it would add greatly to the interest of what he had written. There was also the nautical engineering connected with the subject, which he hoped some one would take up. The main object of the particular form which he had given to this Paper had been to present, as truthfully as possible, the history of the invention itself. Although most admirable treatises had appeared, yet there were few who would go through them carefully, or if they did do so, would have time to glean from them the actual facts which constituted the history of this particular department of science. This was the first occasion, he believed, on which this particular subject had been presented for discussion at the Institution; and he should be glad if, in the course of the discussion, any errors into which he had inadvertently fallen were pointed out.

The Astronomer-Royal said, his practical acquaintance with lighthouses (though he had seen many in a cursory way) began, not as a member of the Royal Commission, but from having been occasionally invited in a friendly way by the Chairman to assist in the examination of the British lighthouses. One point to which Mr. Chance had especially alluded had pressed itself upon his notice, and, no doubt, upon the members of the Commission, viz., that in looking at lighthouses the way to examine accurately their performances was not to look outside but inside. This did not come upon him in full force till he went, accompanied by a member of his family, to look at the Whitby lighthouses, which were pointed out by Mr. Chance as presenting the best instances of British manufacture of the optical apparatus. The moment, however, he came to look at the thing himself, and to have the whole judgment of it himself, he looked a great deal sharper into it than he did before; and he attached great importance to this, for this practical reason, that he did not think the personal organization of the English system of superintendence of lighthouses was good—that of the Scotch system was admirable. However, when he came to take the internal view of things as they presented themselves at the Whitby lighthouses, he saw, to his great astonishment, that the larger portion of the light must be lost. He pointed this out to his companion, who saw as he did; and, what was more remarkable, the attendant on the lights saw it as well. He mentioned that, to show how easy it was to see this great defect in the action of lighthouses, and how curious it was that it should have escaped observation up to that time. The result was, that there was an assembly of persons from several departments at the Whitby lighthouses: the matter was discussed; and he trusted these particular observations, to which Mr. Chance attached some importance, had been of benefit in the subsequent arrangements of lighthouses. He hoped they might operate also in another way, that was, in making an alteration in the personal organization of the superintendence of lighthouses.

He would express his great thanks to Mr. Chance, for giving the history of lighthouses just at the time when a perfect history could be made from the beginning, and especially when the practical question was just in the state for its history to be given. With reference to the shape of the lenses, he might state that at one of the lighthouses—he thought it was the Start—in viewing the formation of an image by the different zones of the dioptric part of the light (as in Fig. 2, Plate 15), it struck him that they must have been ground in the manner in which an optician ground a lens. Everybody knew how convex glasses of any kind were ground in a bowl, and in that way a perfect, but only a spherical shape could be given. It struck him that these different zones must have been all ground in one bowl; and Mr. Chance was good enough to get a note from the man who worked them, which supported that view. The effect of grinding in a bowl in that way was this—that too great a curvature was given to the cross section of the rings of the lens exterior to the centre. He had had the advantage of seeing the beautiful mechanism in Messrs. Chance's works, and that which struck him most was the cross-stroke in the polishing; when there was a ring lens to be made, the cross curvature was not given by grinding in a bowl, but by the cross-work of the polisher; and by some small adjustment of the mechanism, which Mr. Chance had arranged, there was a power of altering the degree of curvature which would be given by that cross-stroke. Upon that everything depended, and he looked upon it as the critical point in the construction of these lighthouses. Now he thought he might say, from what he had observed, that the care in grinding these surfaces had migrated in some measure from its first country to its second. Looking at Mr. Chance's testing methods, he had no doubt that every ring-lens which came from his manufactory was as perfect in its action as it was possible to make anything. Some time ago he examined one of the British lighthouses erected at Paris, and he had no hesitation in saying, the lower reflecting prisms had not been tested in the way practised by Mr. Chance. He had arrived at that conclusion because, when the light diverged from a lamp and fell upon the prisms, the intention was that it should emerge in parallel beams with reference to the vertical plane. Then it was perfectly understood from that, as a matter of optical theory, that the converse proposition held, viz., that if parallel rays came from an object at a very great distance and fell upon these curves, they would converge at the place of the lamp; and that was the foundation of the method of testing to which Mr. Chance alluded, and of which he had spoken in reference to the Whitby lights. In the lighthouse he referred to, which was manufactured for a British lighthouse, and was maintained at the expense of the Trinity Board, he found that the image of the horizon, or that of ships at a distance, was not formed near the lamp, but at about two-thirds the distance between the prisms and the lamp. He need not say that in such a case a large proportion of the light which diverged from the lamp and fell upon the prisms, after refraction by the prisms, diverged ultimately at so large an angle, that it could have only a very slight effect. To this he bore his testimony, and he mentioned, in consequence, his conviction that these points were looked to more carefully in England at this time than in France. There was another point of experience to which he would advert, and he mentioned it as a thing of which he could not give an explanation, because he had not severely examined the dioptric lighthouse concerned. There were two lights at the South Foreland, which could be seen nearly across the whole breadth of the Channel; and he had himself seen them from the gallery of the Calais lighthouse. One of these was a dioptric light, and in the other the old parabolic reflectors were used. On one occasion, crossing the Channel at night, the air being in magnificent condition for observation, he employed himself in steadily looking at, and in comparing the intensity of, those two lights, all the way till he got near Calais. Sometimes he thought one was the better and sometimes the other. He would say, after the pains taken in the general manufacture of the dioptric light, he expected that it would blaze out beyond the other all the way; but the old reflector light was sensibly as good as the new one. From what cause it arose he could not say, for he had not severely examined that lighthouse. In justice to Mr. Stevenson he would now say in words what he had said in print. He had examined a good many lighthouses in England, France, and Scotland, and the best he had seen were the Scotch lighthouses.

Mr. Thomas Stevenson said, after the clear and able Paper by Mr. Chance, he was not aware that there was much, or anything, left for him to say. He begged to thank the Astronomer-Royal for the flattering manner in which he had spoken of the labours of himself and other members of his family.

As to the dioptric spherical mirror, he might say, without derogating from Mr. Chance's merit in the matter, that his (Mr. Stevenson's) first idea was to make it by generating prisms round a vertical axis, but at that time flint glass could not be obtained in large pots. It required to be taken out in very small quantities on the end of a rod, and to be pressed down into the mould. Therefore he was obliged to reduce the diameter of the rings as much as possible; and it was thought by all whom he consulted at the time, as well as by himself, that by generating prisms round a horizontal axis, the more important parts of the instrument would be more easily executed, inasmuch as the prisms at and near the horizontal axis were of much smaller diameter. He mentioned this as an excuse for not having adopted the form shown in the drawing, which was certainly better. Mr. Chance had, however, not only chosen the best form, but had added the important improvements of separating the prisms and arranging them in segments. There was just one other point as to which he would make a remark. The most simple form of holophote was that shown in Figs. 6 and 7, page 511, which was described in his book on Harbours. It consisted of a half-holophote, *a b c*, which operated upon 180° of the flame in front, beyond which angle total reflection could not be carried, with glass of ordinary refractive power. Then, instead of having a spherical mirror, subtending the remaining 180°, it was preferable to have a portion of a paraboloid (*e h*, *f i*), so that the rays were sent forward parallel at once; whereas if the spherical mirror had subtended 180°, a large portion of the rays would have been lost on the burner; and again, all the back rays would have been subjected to the action of more than one agent, whereas by the one paraboloidal agent the incident rays were at once parallelized, and the remaining rays were reflected back through the flame by the dioptric spherical mirror (*e f*). The same might be effected by the use of two refracting and totally reflecting agents of glass; but the one paraboloidal agent was less complicated and nearly as efficient. It thus appeared that the most simple form of holophote was that in which both glass and metal were employed.

Fig. 6.

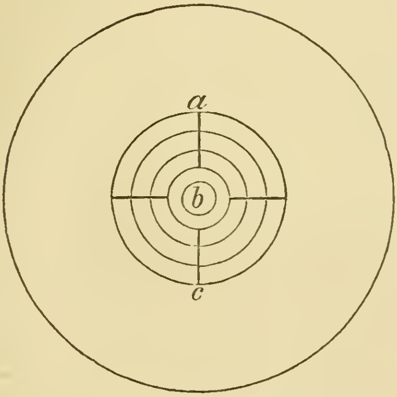
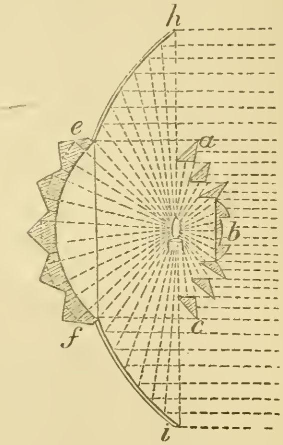
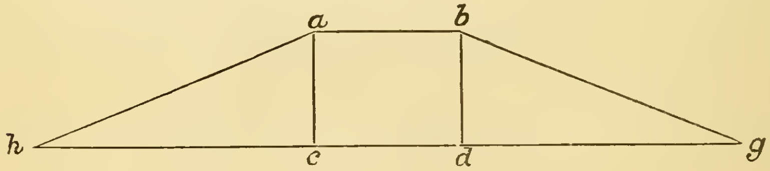


Fig. 7.



Colonel Smith said, he would not have ventured to make any remarks were it not that he thought it undesirable a Paper such as this should go forth without comment. He believed any person who studied the Paper would arrive at the conclusion, that the Author was of opinion that the catoptric system of lighting was so obsolete and out of date, that the sooner it was got rid of the better. He was not an advocate of that system in opposition to the dioptric, because he was a great admirer of the latter; but it was one thing to admire a highly scientific arrangement, which on some occasions was by far the most useful; and another to put aside, repudiate, and discharge with contempt, a system which had proved valuable for so long a time. He had only read the abstract of the Paper in a hasty manner; but the conclusion he arrived at, amongst other things, was that there were about an equal number of lights on the catoptric and on the dioptric systems respectively on the British coasts; and he imagined the opinion of the Author to be, that it was a pity not to get rid of all the old-fashioned apparatus, and substitute the new one in all lighthouses. But he thought the catoptric apparatus, though inferior in scientific arrangement and in its effects to the dioptric, had many advantages, which must not be lost sight of, especially with regard to the colonies. He considered a little undue prejudice had been given to the catoptric system by the remarks contained in the admirable work of Mr. Alan Stevenson, published about eighteen years ago; in which it was pointed out, with regard to both fixed and revolving lights, that the dioptric arrangement was superior to the catoptric in the ratio of 3 or 4 to 1. He had no doubt that was correct; but then Mr. Stevenson took the maximum illuminating power of each apparatus, and omitted all the rest. In regard to the lens, the maximum illuminating power was nearly the whole of it, and one of the drawbacks to the lens system was the very short duration and great power of the flash. In fact, the whole power was concentrated into a cone of 5° opening; but the reflecting or catoptric system was very different. It formed a cone of light, according to the Trinity House arrangement, of more than 19° opening; and comparing merely the maximum power of that reflector—and that was only 4°—and leaving out the 15°, justice was not quite done to it. There were arrangements which might be made for the equalization of the effects of the light, and full justice was not done to the system if they were omitted. Fig. 8 represented the whole useful effects of the parabolic reflector, the heights *a c*, *b d* at various points representing the amount of illumination. Here it would be observed that the figure *a b*, *c d* represented the maximum effect of a parabolic reflector which was perfectly uniform from *a* to *b*; *a h* to *b g* represented the increasing and diminishing effects before and after the maximum.

Fig. 8.

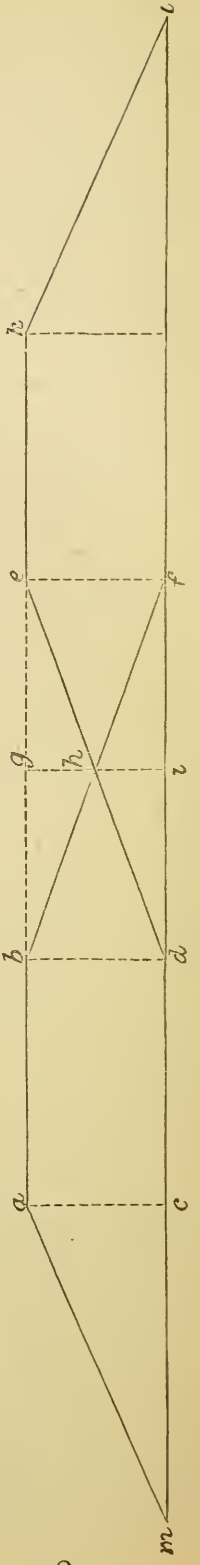


With respect to the irregularity of effect in fixed lights on the catoptric system, mentioned in the Paper and in Mr. Alan Stevenson's book, it was quite true it was irregular, although not so much as was made out, but this irregularity could be in a great measure corrected. If, in Fig. 9, *a b f* and *d e k l* represented the effects produced by two adjoining reflectors, then the triangle *b d f* would show the declining light of one, and *e f d* the increasing light of the other. He must, for the present, assume these to be true triangles; and granting this, if two of these reflecting systems were joined together, so that one triangle overlapped the other (as *b d f* and *e f d*), it did not require demonstration to show, that the quantity of light added by the triangle *d h i* was equal to the quantity of light missing in the triangle *b h g;* consequently the whole effect of the two combined reflectors would be perfectly uniform.

That would be true if the spaces *b d f* and *e f d*, representing the increasing and diminishing effects of the light were true triangles; but they were not so, being rounded by curves at *b f* and *d e*. The consequence was, the overlapping of these figures did not produce an exact regularity in the light, but it was sufficient for all practical purposes. The real objection, in his opinion, to the catoptric light, compared with the dioptric, was reduced to a matter of pounds, shillings, and pence. If as many reflectors could be put side by side around the horizon as would make up a nearly equal distribution, forty-five Trinity House reflectors would be required, which would involve considerable cost, and the quantity of oil consumed would be three times as much as was consumed by one of the Fresnel dioptric zones, to produce the same effect. Supposing the whole circle of the horizon were illuminated, the result would be, as he had just stated, 3 to 1 in favour of the zones; but if only half the horizon had to be illuminated, the central lamp was still required in the dioptric system, and that involved the whole 570 gallons of oil consumption. With the reflectors only 22 might be worked, and half the expense be got rid of. If less than the half-circle were to be illuminated, a smaller number of reflectors would be required. That was one respect in which there was advantage in the use of reflectors.

Again, in the dioptric system, although the central lamp illuminated the whole horizon, yet if anything happened to that light, the whole horizon was in darkness. With reflectors, forty-five or twenty-two lights were required to fill the whole or half the circumference of the horizon, and if one light went out only a 45th part of the horizon was in darkness. However, he was obliged to admit this system was not

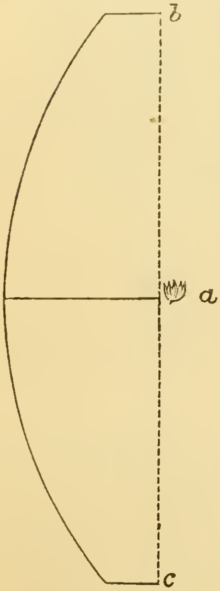
Fig. 9.



absolutely perfect; with all the arrangements that could be made, and with forty-five reflectors, an absolutely perfect light was not obtained; nevertheless he maintained, in opposition to the Author of the Paper, and to what had been previously published, that by other means on the catoptric system it was possible to produce absolute uniformity of distribution, and an equally good effect by reflectors as by zones.

One of the figures exhibited by the Author represented, it was said, a section of one of the lenses and a face of the revolving system; while another represented a section of the zone or fixed system, and the rays of light from the central lamp passed through this section, and were only diverted from their course with regard to their vertical dispersion. Those rays, in so far as they were separated from each other horizontally by radial dispersion, were not touched. Now the same thing might be done with reflectors. Fig. 10 represented the section of a reflector shaped according to the surface of a solid formed by the revolution of a parabola whose focus was at *a*, round the parameter *b c*. The property of such a reflector, as was well known, would be that all the rays from the central focus would be brought to a horizontal line with regard to vertical dispersion, but would be left untouched with regard to radial dispersion.

Fig. 10.



Now if this apparatus were metallic, made of good materials, and of the same dimensions as the dioptric zones; if the light were the same; and if it reflected the rays under the same conditions as the zones which transmitted the rays, would not the effects be the same? He thought they would, but with two exceptions. One was, a ray of light suffered greater loss by dissipation from the surface of the reflector than it did in passing through glass; and another was, that the apparatus in which the light was exhibited was in the way of the rays of light which passed through the centre, so that any pedestal, used to support the light, and the lamp itself would be in the way of all the rays which came from the reflector. That was undoubtedly a great drawback, especially in lights which could not be supplied horizontally; otherwise it might be practically equal to the dioptric system.

One advantage attending this arrangement, which did not appear at first sight, was, that a single instrument of this kind would illuminate the semi-circumference of the horizon, viz. 180°. It could not be made to illuminate more than that, and he was of opinion if it were made of the same size as the zone of M. Fresnel it would do it nearly as well.

Now with a Fresnel apparatus there could not be a second one, but with this there might be two, or more, side by side, and they would all illuminate the same 180° of the horizon; in which there was this advantage, that there would be practically no chance of the light being extinguished. If the chances were 100 to 1 against one light going out, the chance of two going out at the same time was 100 times 100 to 1, and against three being extinguished 100 times 100 times 100 to 1, that was, 1,000,000 to 1. One principle involved in this, and carried out in the other system, was capable of a valuable extension, but he did not see any account of it in the Paper, and he brought the matter forward now in the hope that if it had been carried out the Meeting might be informed of it. It was the principle of separating the vertical from the horizontal divergences.[1] In this and in the dioptric zone the vertical dispersions were taken by themselves; the horizontal dispersions were not touched. Mention was made in Mr. Stevenson's work of a contrivance of M. Fresnel, which he called the application of a cross prism. He had already explained that the system of zones condensed all the rays vertically only, so that if a lighthouse furnished with a system of zones, and a strong light in the middle, were situated in the middle of the sea, there would be a cylinder of light of a depth equal to the height of the zones, and a diameter that of the distance of the horizon on all sides. Supposing this zone were turned sideways, the same effect would be produced laterally as was before supposed to be produced vertically. All the horizontal rays proceeding radially would be collected in horizontal lines parallel with each other as regarded their horizontal relation to one another. The idea of Fresnel was to have a series of prisms, the present system of zones, to remain as now with the light in the centre, and another series of upright prisms outside, a wall of them being so arranged that these should have the effect of collecting the rays laterally, the inner zones collecting them vertically. In order to explain the effect of this combined system of crossed prisms, or zones, he would first notice the effect of the single dioptric series. Now the effect of these zones being to collect the vertical rays only, a spectator a mile off would see at every part, from the top to the bottom of the zones, rays coming from the lamp to his eye as a string of light, of the breadth of the flame; and hence according to the breadth of the flame in the centre, so broad a bar of light would he see from the top to the bottom of that zone. In respect to the joint action of these zones with another system outside it, collecting the rays laterally as well as vertically,—just as the first horizontal set of zones collected all the rays vertically and produced a vertical bar of light, leaving their horizontal dispersion untouched, so the vertical set of prisms would collect the rays laterally, and produce a horizontal bar of light extending from side to side, the whole breadth of the system. The combination of the two would produce a square mass of light, whose area would be equal to the one bar multiplied by the other. Fresnel's first idea was to collect the rays, and thus give a flash, but he had never carried it further. Colonel Smith thought it might be done with the object of increasing the horizontal divergence, the great drawback to the lens system being the short duration of the flash.

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[1] The term dispersion has been used to designate the natural separation of the rays of light from one another to fill the sphere of illumination, such as might be supposed to take place if the light were a single point. Divergence is here used to signify that abnormal deviation of the rays from the proper line of refraction or reflection caused by the angle subtended by the edges of the flame.—J. T. S.

In a series of eight lenses, if each face was the side of an octagon, and the whole system revolved in eight minutes, the flash produced by the lens would only last for $6\frac{2}{3}$ seconds, the darkness occupying the remaining $53\frac{1}{3}$ seconds. That was owing to the lens being so far from the central light, that the angle subtended by the edges of the light was a very small one—5° only. As the lens was brought nearer, a larger angle would be subtended, and the flash would last longer; and therefore it was quite possible, with the cross prisms, inside instead of outside the zones, to bring them to half the distance, and obtain double the length of flash. From a measurement he had made he thought there would be a flash of about 12° instead of only 5°. It would also bring about a little of the effect which was still wanting in the dioptric lights, that of the flash of light beginning gradually, increasing to the full intensity, and then gradually passing to nothing. He would be glad to hear whether anything had been done in this direction, and whether it was considered possible to carry it out practically in the way he had suggested.

Captain Arrow said he did not profess to be a judge upon the scientific portion of the question. His own duties, and the duties of those whom he represented, were, in the matter of optics, confined to calling upon scientific men to provide such instruments as were adapted to each particular case, and to see that those instruments were made use of to the greatest advantage when supplied. At the same time the Trinity House had to provide the best possible organization of the lighthouse system of the country, looking to its necessities in every bearing; but on the purely scientific portion of the question they had to look to those who possessed a skill which he personally laid no claim to. As representing the lighthouse authorities of this country, he considered the dioptric system was a great improvement and a great advance upon the catoptric system. There were many points, not only in its beautiful arrangement, and the way in which the light was economised, which were in favour of the dioptric system, but there was great economy in the system, and, moreover, it added very much to the safety with which the English coasts could be navigated. All would admit that one of the great disadvantages of the catoptric system, setting aside its comparative waste, was the impossibility of using it with the precision which attached to the dioptric system. There could be no better instance of this than was shown on the chart of the Straits of Gibraltar, in lighting which he and his colleagues had taken a considerable part. Thanks to the scientific and manufacturing skill of Mr. Chance, with which he had always been ready to assist the Trinity House; thanks also to Professor Faraday, great advances had been made in the lighthouse system; but in the Gibraltar light there was a peculiar illustration of the advantages of the dioptric system. The Pearl Rock was nearly 6 miles from the mole, and at that distance the red-coloured glass caused so much absorption, that it was difficult to get an effective red light except in the brightest weather. Fortunately, in the Mediterranean the sky was generally bright. The question before the Trinity House was how to utilise this light, so as to guard the Pearl Rock without the cost of a second light upon Cabrita Point, which would have involved a complication with the Spanish Government in addition to the expense. That gave rise to the employment of the beautiful arrangement of the vertical prisms, which by the skill of Mr. Chance had been so adjusted as not only to attain the object desired, a good red fixed light at a distance of 6 miles, but as he was told, on the best authority, the red rays were quite equal to the white at any distance the light was visible. By the accumulation of light from other portions of the arc, the obstruction from the absorption of the rays was so completely overcome, as to make the red light equal to the white.

Another case in point was afforded in the Dartmouth light, Plate 15, Fig. 10. He could say, as a sailor, that it would be impracticable to run up that channel with a bearing of one catoptric light only. There must have been leading lights; but, by the existing arrangement of the dioptric light, the path of safety was distinctly defined; by the shade of red light on the one side of the channel and of green on the other, the ship's course was clearly pointed out. The green light was in itself a bad colour, but it was the only one that could be adopted there. One light thus did the duty of two. The lamp was a little bigger, and a somewhat larger quantity of oil was burnt than in the ordinary Argand lamp; yet that was made to serve the purpose of two lights, and was sufficient to define with the greatest nicety both sides of the channel, and was of the greatest practical benefit. Not long ago he anchored in Dartmouth, after which the weather came on thick and misty, though not sufficiently so to obscure all light. Under that condition of the atmosphere, he got the vessel under way again, ran her out, and doing his duty as a lighthouse officer, he tried these lights on both sides of the channel, touching the edge of the red and green alternately to satisfy himself that they were acting properly, and he navigated his vessel in a thick night with the greatest accuracy. That was a practical demonstration of the economy which resulted from the use of the dioptric light.

Some observations had been made with respect to the catoptric system, which were worthy of consideration. An immense volume of light proceeded from the catoptric light when burnt upon one plane, and when it could be shown in one beam it was very good indeed; and, to the best judges, it was difficult to say whether the revolving light of the dioptric system surpassed it. He believed he was right in saying he did not think there was a revolving dioptric light which surpassed two or three of the catoptric lights existing on the English coast. In the Beachy Head light, for instance, there were thirty reflectors on three faces, and upon each face there were ten lights set in one plane, showing a magnificent beam of light, with a lengthened and protracted flash, which was very useful, particularly in thick weather, when the difficulty was to catch sight of any light. If the flash were prolonged, so much the better; he need not say to those experienced in these matters that to pick up a light at all required a great degree of practice. He had stood on deck watching for lights up channel, and had looked and looked till he fancied he saw them, but it was a sort of *ignis fatuus;* and for this reason, if the flash could be prolonged without weakening its power, it would be of great value, as giving so much more time for the eye to receive the rays. He held, as the representative of the lighthouse authorities, that the revolving lights on the catoptric principle were so good at this moment, that it would require much consideration to disturb them, particularly where so great an amount of light was given, as in the instance he had mentioned of Beachy Head. There was no doubt economy in the dioptric lamp, but not to the extent stated. The tendency of the evidence given before the Royal Commissioners was, however, to the effect that the catoptric system of lighting, as carried out in this country, was a very powerful source of light. The Trinity House authorities were acting at this moment upon that view of the matter; and, while they were changing the fixed lights as speedily as they could to the dioptric system, they had no intention at present of changing the larger revolving lights, because it was doubtful whether it was possible to improve them much at the present time; but the time might come when a different opinion would be entertained. The present was an age of progress, and no one could tell whether experiments that were successful to-day might not be eclipsed to-morrow.

It appeared to have been a source of reflection against the Trinity House and other Boards that they moved slowly; but when they did move it was generally in a safe groove. Here was a case in point. It was mentioned by Professor Airy, that the relative power of the two South Foreland lights—one being dioptric, and the other catoptric, was undistinguishable; and in his voyage across the Channel, he was puzzled to decide which was the better light of the two, and he naturally laid that to something in the dioptric light which deprived it of the superiority it ought to have had. This was actually the case. It was a particularly old-fashioned lantern, which detracted from its value, though it did not detract so much from the value of the catoptric light. If that lantern had been changed two or three years ago, the whole work would now have to be gone over again, because at the South Foreland it was the intention of the Trinity House to place immediately electric lights in both towers, which would require different lanterns from those commonly used, and different arrangements, compelling a change altogether. Therefore, if this change had been made at an earlier period, it would have entailed to a certain extent a loss of money. He mentioned this circumstance to show, that the authorities could not move so rapidly as some people appeared to think they ought to do. As a public department, the lighthouse authorities did expect criticism upon the way in which they carried out their duties, and they were perfectly ready to meet anything in the shape of fair criticism; but he thought there ought to be some allowance made, and some credit given for the difficulties which the Trinity House had to struggle with, in having had to work a system of comparative antiquity. It was for a long period of time far in advance of that under the control of any other lighthouse authority, whether in Scotland, Ireland, or France; and therefore, being so far in advance, it was the more difficult to alter. On the other hand, the French jumped at once from the most insignificant lighthouse arrangements to a degree almost of perfection. They had no shipowners to consult; these matters were done in France with a stroke of the pen: an alteration was ordered, and it was made forthwith. But in this country nothing of the sort could be done. Those for whom these duties were administered did not find means for making unlimited experiments, nor would they consent to throw away a good thing for the sake of a better, unless it was proved that the old one was not good enough. From some of the remarks that had been made, he thought a little undue stress had been laid as to the improvement that was wanted in the organization of the Trinity House. He could understand that, to a person highly versed in optical science, in his view optical science was the great thing wanted. Perhaps the next criticism would be directed to something in connection with the construction of the lighthouses, or buoys, or lightships, all of these calling into play other branches of science. But it would not do to confine the organization to one of an optical, or of any special character only. The lighthouse authorities of this country had striven to make available every branch of science, so as to be able, in carrying out the details, to make use of all the appliances which science had placed at their command. Great thanks were due to the Royal Commissioners who sat some eight or nine years ago. They had, with much perseverance and pains, and he had no doubt at a large expenditure of money, accumulated an immense number of facts. He did not agree with all the conclusions they had arrived at; but he approved of a great deal they had done. To Professor Airy was due, he believed, the discovery of a means of effecting a more perfect adjustment of the optical apparatus, which the Royal Commissioners had adopted, the knowledge of which was previously unknown to the Trinity House, as he believed it was unknown to all other lighthouse authorities. The same defects existed in the lights in Scotland as in the lights of the Trinity House; and in the strictures which the Royal Commissioners had made the very worst light that was shown as being improperly arranged was a Scotch light. He believed the Royal Commissioners were under the impression at the time that it was an English, and not a Scotch light, but it was the worst on which they put their finger; so that the Trinity House did not stand alone in this, if it was a blot in the system. Thanks to Professor Airy,—thanks to the use made by the Royal Commissioners of his observations, he was glad to say the inquiry had resulted in public good. It only wanted to have the knowledge of the defect brought to light to induce Professor Faraday to set to work to rectify it; and when he said they had the advice of Professor Faraday, it would be admitted that they had appealed to a high and competent authority. Professor Faraday invented a small instrument which rendered it easy, with the most ordinary skill, to adjust any of the existing dioptric lights. Since then much attention had been paid to the subject, and, with the advantages of Messrs. Chance Brothers' manufactory, and Mr. Jas. Chance's mathematical skill, in conjunction with Professor Tyndall, whose scientific attainments had been of great use, he did not think it probable there would be any just cause of complaint, so far as the organization of the Trinity Board went, in respect to the question of optical skill. The lighthouse officers of the kingdom did their best; they never turned a deaf ear to any suggestion where they saw an opportunity of advancing in the right direction. The Royal Commissioners were armed with great powers, and had the means of arriving at facts which without those powers it would have been impossible to obtain; and they certainly had called attention to defects common to all lighthouse authorities. Since then great improvements had been made in optical apparatus. The vertical prisms were no doubt a step in the right direction; but there still remained much to be done, and he hoped to see improvements introduced which would diminish the cost of the dioptric lights, and make a smaller lamp do even greater work than the large ones did at the present moment.

Mr. R. P. Brereton remarked that he had had experience of Mr. Chance's great skill in the manufacture of optical apparatus for lighthouses, in the case of one constructed by him at Kingsweare, for the Commissioners of Dartmouth Harbour, the object of which was to give a sea-light, as well as to define distinctly by different colours the fairway channel to the harbour between the 4-fathom lines at low water on either side. The harbour entrance between those lines was narrow, and, as a white or leading light of only 9½° was required, it was necessary, to correct divergence of the rays over so small an arc, to have perfect appliances and very careful adjustment; and, from his own knowledge of the navigation, he could state that the objects in view had been remarkably well accomplished. The light, Plate 15, Fig. 10, was in itself simply a fourth-order dioptric light, with the refracting belts and reflecting prisms of the ordinary kind, and the whole of the light was directed seaward. From the land-locked nature of the harbour and the high ground adjoining, the direct light visible from the sea embraced only 45°, of which 9½° were white light, the sides being green and red. To intensify the rays of separation, or of transition between the bright and coloured lights, Mr. Chance had introduced vertical prisms of total reflection, five on one side and five on the other: four of these prisms condensed the light upon the edge of the red and four upon the green. The two outside prisms were for the increase of the direct bright light to sea. These vertical prisms were placed outside the illuminating apparatus, and the coloured shade shown by the green and the other by the red. Captain Arrow had remarked that green was a bad colour, but Mr. Brereton found, in coming up the channel, that though the green light was not so distinctly seen as the red and white lights, still the object was attained by navigators, who, when they ceased to see the bright light and did not catch the green, knew they may be getting into dangers, the whole of which, however, lay within about a mile and a half off shore, where the colours were distinctly seen. Although this was only a fourth-order light, it had been seen in clear weather at a distance of 18 to 20 miles, which was nearly equal to a first-order light.

Dr. Gladstone remarked, that though he had listened to Mr. Chance's elaborate Paper critically, there was nothing in it which he could find fault with; therefore the few observations he should make would only be supplementary.

First of all, he would say a few words on the question of catoptric and dioptric arrangements. Mr. Chance had stated that the dioptric was superior to the catoptric, and he had laid it down, that the best way of judging of the comparative merits of the two systems was to go to the French comparisons. Though Mr. Chance did not give these comparisons, still they were stated to be in favour of the dioptric arrangement. He had no doubt it was so. But it was scarcely fair to the English lighthouse keepers to compare them with the French, for this reason, that he never knew a French keeper who could polish a silvered mirror well; and the polishing of the mirrors was a most essential matter in the catoptric arrangements.

The question had been ably discussed on many points, but it seemed to him to admit of some further consideration. It had been said that the Fresnel lamp could not be multiplied, that the four-wick lamp was as large as it could be, and that but little improvement could be made in the optical arrangements; but the Argand burners and mirrors might be multiplied *ad infinitum*, and thus a brighter light might be produced by the catoptric than by the dioptric arrangement: and this might explain some of those curious anomalies which at first seemed unintelligible. Thus there were Professor Airy's observations, that the two lights of the South Foreland appeared to be very much alike, which, indeed, they were: then there was the fact alluded to by Captain Arrow, that on an examination of the lights by the Royal Commissioners, they came to the conclusion that many of the catoptric revolving lights, including that at Beachy Head, were the most beautiful exhibited. The reason was, the mirrors could be multiplied, and there might be thirty Argand burners, ten at once sending rays all streaming across the surface of the ocean in the direction wished; and if ten were not sufficient, there might be twenty or thirty burners, all sending their rays in the same direction: but it could of course only be done at a largely increased cost; and the Beachy Head light consumed 1,000 gallons of oil annually. Another advantage of the catoptric system was that it helped to afford the required distinction between different lights on a sea-coast. Mr. Chance had shown the great distinction between fixed and revolving lights. Dr. Gladstone had not much love for fixed lights, inasmuch as they were easily confounded with lights on shore or on board ship; and whereas a fixed light was always the same, many different kinds of lights might be made to revolve at various rates of speed, and to display various alternations of colour, as in some of the most beautiful lights in France and Great Britain. Then, again, there might be short flashes and prolonged flashes. With an electromagnetic apparatus the line of light was a sharp line which rapidly passed away, and at a distance there was scarcely any light excepting at intervals, when there was a sharp flash, rapidly disappearing; whereas with the large mirrors at Beachy Head, there was always a strong light; but this light once in every two minutes gradually increased to its maximum of brightness, and then diminished again. In this respect there was an appreciable difference between a revolving catoptric and a revolving dioptric light. The general result he arrived at was, that without doubt on scientific, practical, and economic grounds, the dioptric light was, on the whole, the best; but there were advantages connected with the reflectors which must not be lost sight of, and he thought the authorities acted wisely in retaining those beautiful lights, which had been alluded to, on the British coast. Now that the better adjustment of the glass apparatus was introduced, he had no doubt the dioptric system would show its great superiority. On the subject of adjustment, he would say that the Royal Commissioners learnt from the evidence of many witnesses, that the light at Cap Grisnez was a very beautiful light. When the Commissioners went to France and saw the lighthouse authorities there, they expressed a wish to visit and inspect that lighthouse; but they were told that, inasmuch as it was an old-fashioned light, and one of the first that had been made on the lens system, they need not trouble themselves about it. They went as advised to Cap D'Ailly and Calais, where there were very beautiful modern lights, but they also did trouble themselves about Cap Grisnez. And here he must go a little into the history of the thing, as it had not been quite correctly represented. The secretary of the Commission, Mr. Campbell, being a photographer, thought of this principle of internal observation for the purposes of adjustment of the apparatus. When he mentioned it to the Commissioners, they at once saw the value of the suggestion, and it was applied in various ways; and this method of observation was employed at the Start light, and subsequently at Cap Grisnez. It was found with astonishment that the latter was beautifully adjusted, and there were reasons for believing that M. Fresnel had adjusted it somewhat upon the principle now adopted. That solved the secret of the beauty of the light at Cap Grisnez: while the upper and lower mirrors were only quicksilvered ones of a very poor description, the whole was so well adjusted as to send a beam of light right over the English Channel. This tended to give the Commissioners additional confidence in the importance of correct adjustment, and they pointed it out at the North Foreland to the Elder Brethren of the Trinity House, to Professor Faraday, and other lighthouse authorities. It was after his visit to the Start, in conjunction with the Royal Commissioners, that Professor Airy visited the Whitby lights, where, as he said, he first woke up to the importance of the method of internal observation. Thus it was not he who first brought this plan before the notice of the Royal Commissioners; on the contrary, it took some time to convince him of the importance of the matter. It was, however, now appreciated by all parties, and to Professor Faraday they were indebted for some of the practical arrangements for carrying it out.

He would add that all these beautiful arrangements required not only good adjustment, but that all the parts should work together to make the light as perfect as possible. In inspecting one of the lights devised by Mr. Stevenson, in one of the narrow channels on the west of Scotland, it was found that there was an astragal in the way of the most important beam of light. In passing in front of the harbour light of a principal seaport, the light was found suddenly to disappear, and then appear again. On visiting that light it was found that in front of the apparatus, which was a very good one, there was a pillar 4 inches or 5 inches broad just in the most important part, and the inspecting party had been going through the dark shadow cast by this pillar. The practical observations of Mr. Chance with regard to divided responsibility in these matters should be borne in mind. In operations like this all should work together, and there should be perfect harmony of action between those who planned the lights and those who constructed or erected them.

Admiral Hamilton, chairman of the Royal Commission on Lights, Buoys, and Beacons, said the Commissioners found on examination, that the Trinity House had endless difficulties to contend with; and under such circumstances it was evident the system could never work properly. If the Trinity House made suggestions, these had to be laid before the Board of Trade, who looked too exclusively at the economical side of the question, and too frequently said "No," without the intervention of any scientific person, and the Trinity House were hampered in every way. Looking over the report of the Royal Commissioners, for the first time for many years, he adhered to everything it contained. He believed the Trinity Board had taken that view which Captain Arrow gently touched upon, that the Commissioners had been hard upon them, and had not done them justice in their report; but he was glad of this opportunity to refer to a paragraph of the report which he thought entirely took away the sting which Captain Arrow had hinted at; and if he got the support of the Royal Society on one point recommended in that report—if the Trinity House would take it as it was meant, and not view it as reflecting on their administration—it would be for the advantage of science, he thought, that that recommendation should be carried out. Captain Arrow, in his remarks, assumed that optical science was paramount with the Commissioners. In some sense that might be true; but so far from ignoring the other branches in connection with this subject, he was sure that if Captain Arrow would but turn to that section of the report under the head of "Quality of lights," extending from page vi. to xii., he would admit that a vast deal more was touched upon than the mere optical question, and that the immediate practical points connected with the objects of lighthouse illumination were borne in mind.

He would also call attention to the 19th paragraph of his own letter, in which he said:—"And in order to satisfy the public that our Lighthouses, and the whole system of Lighthouse illumination, are in all respects what the highest state of science can produce, and the interests of this great maritime country require, the Queen might be advised to issue her warrant appointing the President and Council with other Fellows of the Royal Society annually to visit the central establishment at the Trinity House, as is now the case with the Royal Observatory; and that the Trinity Commissioners for Lights should on that occasion submit a report of their proceedings in all matters relating to the development of and improvement in Lighthouse illumination to the Visiting Board of the Royal Society; such report to be presented to Parliament with the annual estimates."

The Commissioners advised that the Trinity House should have placed at their disposal and service the assistance of men of science, and that the constructive details should also be carried out under their direction and superintendence. He could not say how much the Royal Commissioners were indebted to Mr. Chance. The Trinity House, and others who were interested in the maritime concerns of the country, were aware how much was owing to him, and Admiral Hamilton considered himself fortunate in having been at the head of an inquiry in which the services of such a man as Mr. Chance could be made available. Allusion was made in the report to the superiority of the building of the lighthouses. There were none superior to those built by the late Mr. James Walker (Past President Inst. C.E.), and he greatly regretted that the name of that eminent man in connection with them had been omitted. In conclusion, he would say that the primary object on the part of the Royal Commission had been to assist the Corporation of the Trinity House as much as possible.

Captain Arrow assured Admiral Hamilton that his allusion to the Royal Commissioners had not been made in any unkind spirit; but he thought the remarks of Professor Airy were of a character rather disparaging to the lighthouse authorities and their officers, and it was to those remarks he had alluded. He never meant to cast the slightest reflection upon the Royal Commissioners.

Mr. Babbage observed, that he had but little to say on this subject, except to express, in common, he believed, with all who heard it, how highly he appreciated the excellent Paper, and the equally admirable tone in which the Author's explanation of it was expressed.

Of course it was impossible that all the details could be read at one meeting; and consequently questions were asked on subjects, which he had no doubt would have been answered by the Paper itself. He had himself no criticisms to offer on the subject; but he thought this beautiful apparatus was capable of being rendered of still greater utility. Most lighthouses were upon the revolving principle, some revolving with more, and some with less velocity; and others had temporary eclipses; but there were circumstances which were greatly influenced by the state of the weather, in certain conditions of which there was difficulty in estimating even roughly the distance of a lighthouse from a ship. There was another way in which these lights might be utilized, still, however, preserving them for their primary purpose of lighthouses: he alluded to the adoption of a system of signals, which in some cases might be of the greatest use; and he believed that it could be accomplished without any diminution of the intensity of the lights, or for the primary purposes for which they were required. By this plan, which occurred to him fifteen years ago, he produced occultations, and the system of signals he proposed would be governed by the number of occultations, and by the amount of light intercepted by each. Seeing this beautiful instrument, it occurred to him that it might well be employed for that purpose. During the Exhibition of 1851, he had an occulting light placed on the roof of his own house, with a view to experiments upon its adaptability for telegraphic communication; and subsequently he received a communication from America, requesting him to visit that country to establish that system there.

He thought he might be permitted to state a curious fact relating to the effect of these very rapid occultations upon the mind. He had produced occultations so quickly succeeding each other, that he was aware of their being double occultations before he was enabled mentally to put into mental language the expression of that fact. This system of rapid occultations appeared to him capable of adoption for signalling purposes; and he thought that in the hands of the manufacturers of these instruments, it might be turned to some practical account. He had published an account of this system fifteen years ago; and yet, such was the state of the Government of this country with respect to scientific matters, he understood the Government had adopted the principle without the slightest acknowledgment of it in any way whatever. As an Englishman, he would say he thought it was an unworthy thing for persons at the head of affairs to do that in their official capacity which would justly be esteemed disgraceful if it were done for personal ends.

Mr. Jas. N. Douglass said, the first order apparatus of the Gibraltar lighthouse for a fixed light with vertical condensing prisms, to which Mr. Chance had referred in the Paper, was an instance of care in design, great perfection in the material of the glass portions, and optical accuracy in construction. To show with what precision this apparatus was erected by Messrs. Chance he might state, that considerable improvements were made by the Corporation of the Trinity House, at this lighthouse, in 1863-4, and it was determined in designing the new dioptric apparatus, which was to illuminate 288°, to have in it an arc of 23° of very powerful red light, for the purpose of marking the Pearl Rock Shoals off Cabrita Point, at a distance of five miles from the lighthouse. The work was so accurately performed, that when the light was tested by a Committee of Elder Brethren of the Trinity House, the line of demarcation between the white and the red lights at the Pearl Rocks was found to be identical with that determined on, and not the slightest alteration was required in the adjustment of the apparatus.

He had had many opportunities of observing from the sea the relative illuminating power of the two lights at the South Foreland; and he had noticed, like the Astronomer-Royal, that on some bearings at sea the upper, or dioptric, appeared, as it should do, the more powerful, and on other bearings the lower, or catoptric, seemed to have the advantage. He would briefly describe the illuminating apparatus in each lighthouse, and the reason, in his opinion, for the observed difference in illuminating power on certain bearings. The low lighthouse had fifteen paraboloidal silvered reflectors, 21 inches in diameter, and Argand lamps with $\frac{7}{8}$-inch burners. The apparatus illuminated a sector of sea surface of 199°, giving 13° 16′ as the work of each reflector; therefore, as the reflectors had each a divergence of 15°, the light should be practically uniform in illuminating power throughout the above sector. The high lighthouse had a first-order dioptric apparatus for fixed light, manufactured by Lepaute, of Paris, in 1841. It was composed of six 45° panels of refractors, two 45° metallic spherical reflectors, and nineteen zones of prisms, thirteen upper and six lower. The apparatus illuminated a sector of sea surface of 270°. The apparatus and lantern, like all those of French manufacture, were constructed with vertical and horizontal framing, the vertical portions opposing at every $22\frac{1}{2}$° an obstruction of the light equal to about 40 per cent. This obstruction was so much increased, when the compressed intense illumination of the electric light was used, during the experiments that were made by the Trinity House with this light in 1857-8, that the officers of steam-packets crossing the Channel were known to have used the partially illuminated bearings for checking their position at sea.

As was stated in the Paper, the illuminating power of a fixed dioptric light was about only $\frac{1}{8}$th that of the best revolving dioptric light of the same order; it was, therefore, of the utmost importance in a fixed light, that the whole power of the apparatus should be sent to the mariner practically unimpaired by obstruction of framing throughout the whole illuminated sector of sea surface.

To the late Mr. Alan Stevenson was due the introduction of inclined framing for the apparatus and lantern, for the purpose of reducing the obstruction of light, and distributing the reduced obstruction nearly equally over the illuminated sector of sea surface. Inclined framing was also adopted by the late Mr. James Walker; in both instances the lanterns were framed with gun-metal, and glazed with flat plate-glass. In designing lanterns for the Trinity House, he had lately, with the view of reducing the obstruction of the light to a minimum, as well as that of the aberration of the rays proceeding from the single lamp of a dioptric apparatus, adopted the cylindrical form for the lantern, with steel for the framing, the latter having an inclination of 30°, and helically curved throughout. A first-order lantern of this description was shown in elevation and section in Plate 16, and a small model of the glazed portion and its framing was also exhibited. Before constructing one of these lanterns, he had made some experiments with various forms of framing, in which he had the valuable advice and assistance of Professor Faraday, the scientific adviser of the Trinity House; a section of the framing, full size, was placed before a first-order dioptric apparatus and lamp for fixed light, and it was found that no visible shadow was cast upon a white screen placed only 45 feet from the light. Professor Faraday reported on the experiment as follows, "A full-sized model of part of a lantern on the construction of Mr. Douglass was placed before the lamp and optic apparatus. It cast no sensible form of shadow on the screen. In fact, the whole amount of shadow was a minimum, and it was uniformly (or nearly so) diffused over the illuminated interval. I conclude that it would be nearly a matter of indifference (as regards shadow) where the uprights of the optical apparatus were placed in relation to such a lantern."

The framing of the lantern was of puddled steel, having a tensile strength of 32 tons to the square inch; it was rolled in halves, as shown in sectional plan at E F, Plate 16. The quadrilateral and triangular frames were welded together at the top and bottom corners, and were adjusted to size and curvature on a gauge block: they were afterwards fitted and riveted together, each pair of quadrilateral frames fishing the junctions of the frames above and below them, and thus forming a rigid framing, of nearly uniform sectional area and strength throughout the lantern, for the support of the plate-glass. The framing was riveted together at the workshop into eight triangular sections the whole height of the lantern, leaving only eight joints to be riveted up at the lighthouse. The rebates of the frames were all carefully adjusted to standard templates, to which the plate-glass was manufactured by Messrs. Chance, so that the panes of glass might fit throughout any lantern of the order to which they belonged. The fillet in the sash-bar to receive the glass was parallel with the front of the sash-bar. The glass was not flat, forming a structure in facets as in lanterns previously constructed; but it had the necessary curvature given to it, so as to form, when glazed in the lantern, a true cylinder, affording the greatest degree of optical accuracy for the transmission of light from the central illuminating apparatus. The glass was secured in position by gun-metal cappings screwed to the framing. A supply of spare panes was kept at each lighthouse, so that in case of accident to a pane, it might be immediately replaced by the light-keepers. Several of these lanterns had been erected, and one was exhibited by the Corporation of the Trinity House at the Paris Exhibition, with a first order dioptric apparatus in position, manufactured by Messrs. Chance.

A cylindrical lantern specially designed by Messrs. Chance for their dioptric apparatus, in which the English electric light was exhibited at the Paris Exhibition, might also be seen on a lofty temporary scaffolding in the Park. This lantern had horizontal, vertical, and inclined framing, nearly all of which was made to coincide optically with the framing of the dioptric apparatus for fixed light.

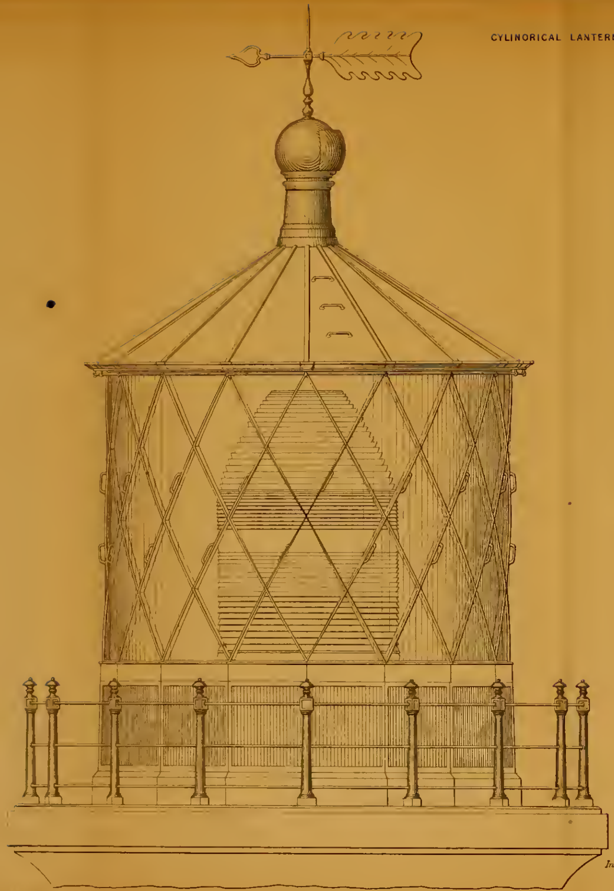
He might extend his remarks on the first order cylindrical lantern, to the special arrangements for ventilation, to meet Professor Faraday's views, for distributing the upward current over the internal surface of the lantern glass, and thereby preventing condensation thereon; but, as he was about to prepare a Paper for the Institution on the construction of lighthouses, he would in doing so go more fully into that subject.

Mr. C. W. Siemens said, it had been objected that the Paper was not of an engineering character, but the subject was intimately connected with engineering, and had been received with interest by the members. Mr. Chance had confined himself to the optics of lighthouses, which was a large subject by itself, although many would have liked to have heard about their mechanical construction, on which he had so much practical experience, and also on the constitution of the glass, which Mr. Siemens believed was of great importance to the results obtained. The description of glass used in the lenses and prisms was, he understood, generally flint-glass—that was glass which had oxide of lead for its base; but this glass varied very much in quality. A small addition of lead would increase its refractibility considerably, and he knew there was difficulty in getting an even mixture at the top and bottom of the glass pot. He therefore thought there must be some special means of obtaining uniform refractibility, or some ready means of adjustment for differences in the degree of refractibility, which he would ask Mr. Chance kindly to explain. One point of great interest had been touched upon, which should be fully discussed. The Astronomer-Royal, in going from Dover to Calais, observed that at a certain distance from the two Foreland lights, one dioptric and the other catoptric, the two showed no essential difference in intensity, though the dioptric light was far more brilliant than the other when viewed from a short distance. No explanation of this observation had been offered, and he would merely suggest whether it might not be the case that, although the dioptric light was the more brilliant in itself, it would nevertheless, at a considerable distance, produce the same effect only as the other light for the following reason: If light might be regarded as a vibratory motion of the medium through which it was transmitted, any obstructive matter in the form of haze or smoke must exercise a destructive effect according to the square of the energy of vibration, or intensity of the light. If that were the case, it followed that a brilliant light would in an obstructive medium soon subside into a light of moderate intensity, and thence proceed at a more equal rate of diminution with light proceeding from a less brilliant source but of equal magnitude, the latter being chiefly determined by the extent of light-emitting surface. For instance, one light produced by a candle would be lost sight of, under certain atmospheric conditions, say at a distance of half a mile. But with six lights of the same size placed side by side, a sufficient amount of light would be conveyed to that distance to produce a distinct effect on the eye. In the same way the glare of the gas-lights of London was seen at a distance of twenty or thirty miles, whereas a limited number of more intense lights would be lost to sight at that distance. He therefore thought the quantity of light emitted was of more importance than its intensity in seeking distant effects, a circumstance which had not perhaps been fully considered in estimating the relative value of the electric light, as contrasted with the ordinary optical apparatus of extended surface.

The question had been put, whether the dioptric light was, under all circumstances, better than the catoptric; and the Author of the Paper seemed to be much in favour of the dioptric system. Now it appeared to him that, for lights of comparatively short range, the catoptric system could be used with advantage, because the reflecting mirror was the more simple arrangement; and if its surface could be kept clean, it would reflect the light in a certain definite direction without much loss, provided the parabolic

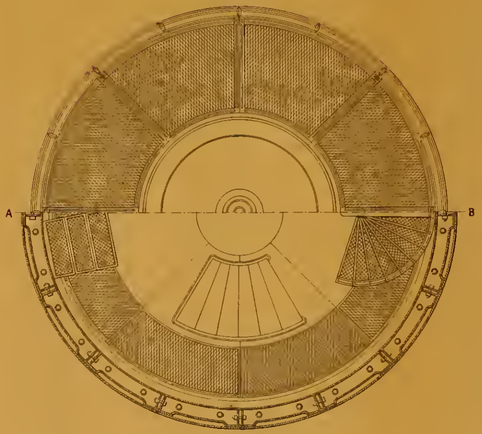
OPTICAL APPARATUS OF LIGHTHOUSES.

CYLINDRICAL LANTERN FOR LIGHT APPARATUS OF THE FIRST ORDER, CONSTRUCTED WITH HELICAL FRAMING OF ROLLED STEEL.

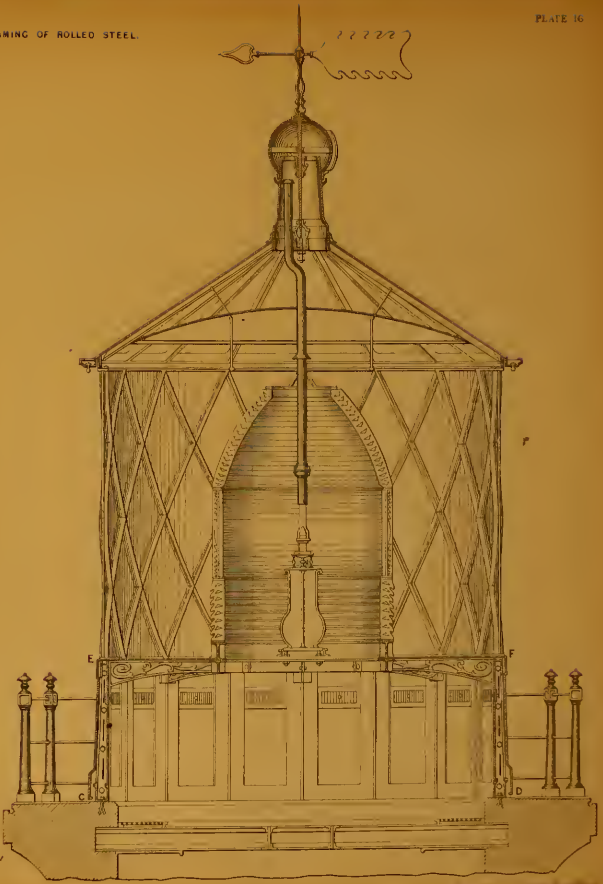


ELEVATION.

PLAN AT E F.

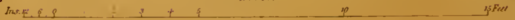


PLAN AT C D.



SECTION AT A B ON PLAN.

Scale.



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KELL BR LITH [illegible]

mirror were extended far enough over the light. The principal drawback appeared to be, that the surface of the parabolic mirror became tarnished; and in order to prevent that, he would recommend those interested to try pure nickel surfaces, produced by the galvano-plastic process. He had tried them, and he thought they were perhaps of all metallic surfaces the least apt to tarnish. Nickel was as hard as hardened steel, and it seemed to remain perfectly bright under all atmospheric influences, even in rooms where sulphuretted hydrogen was present.

There was one other light, which had occupied his attention during the last twelvemonths, to which he would refer:—Mr. Thomas Stevenson, of the Northern Lights, had proposed to establish flashing lights (that was to say, lights giving out flashes at certain intervals) upon beacons and buoys; and Mr. Siemens had been applied to with a view to accomplish that object. The source of light was to be upon the land, because there were periods of the year when a landing could not be effected with safety at the beacons or buoys; and the source of light which naturally suggested itself under these circumstances was electricity. The apparatus that had occurred to Mr. Stevenson was the Ruhmkorff coil placed upon the land, and communicating with the beacon through a cable; but the preliminary experiments at once showed, that the discharge of a Ruhmkorff coil would be absorbed in a cable of only 100 yards in length, and that no spark would be produced on the beacon. The next thing tried was to place the coil on the beacon, and to send simply the battery current through the cable: a cable having a large metallic section was taken, but nevertheless the absorption was such, that no perceptible spark could be produced. Under these circumstances the idea suggested itself to him, that a simple metallic circuit might be established through the coils of an electro-magnet, and that the extra current produced in breaking that circuit would produce a flash, close to the electro-magnet upon the beacon, which would be increased rather than otherwise by the accumulated charge in the connecting cable. If this could be practically accomplished, then the light might be placed at a considerable distance from the shore, without destroying the battery effect which had to be transmitted from the land through a cable. The apparatus was not perfected at once; but he had placed one on the table which would accomplish the object in view. It comprised a heavy electro-magnet, the coils of which were supposed to be in communication with a battery on land through a cable. A clock-work apparatus on land established the electric circuit through the cable at certain predetermined intervals. The electric circuit through the cable was, however, not complete, unless the weighted armature of the electro-magnet was in its distant or unattracted position. The attraction taking place, the circuit was broken at the point of a platinum pin, which was drawn from a mercury bath, and a brilliant discharge of extra current ensued. The current being thus broken, the armature fell back and re-established the circuit, when it was again attracted, and a discharge again took place, and so on during the periods of time when the circuit was established on land. The mercury was continually renewed at the point of contact by means of a circulating pump, which was worked by the electro-magnet itself, which latter had to be very powerful in order to produce an intense light in its discharge. The point of discharge was placed in the focus of a dioptric or catoptric reflector, upon the beacon or buoy to be lighted. This apparatus had only lately been completed, and had not yet been tried at sea; but it had been at work experimentally for some time, and appeared to give very constant effects. If this apparatus was constructed for throwing the light only through a limited arc, the effect would be much intensified; and in that form he thought it might be placed with advantage at narrow entrances, where each light would tell its tale by the periods of successive flashes peculiar to itself; and since the succession of flashes could be varied at will by the contact arrangement on land, the apparatus might also be used for conveying special warnings or signals to vessels out at sea. This apparatus was only applicable to a succession of flashing lights.

Mr. Campbell, Secretary to the Royal Commission on Lights, Buoys, and Beacons, said his knowledge of the subject was chiefly gathered during the time that the Royal Commissioners were at work, between 1858 and 1861; he had not considered much about it since the report was presented, but he understood that there had subsequently been great improvements. He thought, however, he could explain why it was that the dioptric was not better than the catoptric light at the South Foreland, at the time when those two lights were observed and compared by the Astronomer-Royal. The dioptric apparatus was not then adjusted so as to make the best of the light produced by the lamp, and the flame of the lamp was inferior to flames observed elsewhere. The apparatus was constructed to throw light horizontally, but at the South Foreland it was placed as high as 372 feet above the sea. The flame itself was small and low, and consequently observers on board ships crossing the channel saw chiefly the points of the flame through the glass of the apparatus. The flame was inferior to begin with, and the best of the light produced by it was thrown on the sky far above the visible sea horizon, distant 20 miles. On looking through the glass apparatus from within, the sea horizon was not seen by Mr. Campbell from the proper places through different parts of the apparatus.[1]

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[1] For an account of observations made by the Commission which bear upon this point, see Report, vol. i., p. 49, pp. 52, 53, where illustrations are given of the

Now, if the apparatus had been properly adjusted, he believed that this light would have compared favourably with the other light; but he could not speak to the fact from his own knowledge, for he had not, since June 20th, 1860, compared the two lights from the sea.

Admiral RYDER, a member of the Royal Commission on Lights, Buoys, and Beacons, said he had no doubt but that great pains had been taken to prepare the beautiful dioptric lights of Mr. Chance, for England, Ireland, and Scotland. In Great Britain, where intelligent observation was applied to them, they did their duty well; but in the colonies, a different state of things obtained. The same kind of instruments were sent out; but when they arrived, from ignorance on the part of the people who had to do with them, they were mismanaged to a great extent. He would mention the state in which he had found a first-class, revolving, dioptric light, at Bermuda. The island was surrounded by rocks to a great distance on one side, and it was most important that this light should do its duty thoroughly. On approaching the island, he saw the light at a considerable distance. The ship was anchored in the basin—a distance of about 4 miles from the light—on the next night after his arrival, and he was anxious to watch the light previous to the visit he proposed to make to it. To his great surprise, after it was lighted, no flash was visible from the deck of his vessel, the 'Hero,' a line-of-battle ship. His eye was about 34 feet above the water. He thought at first that the keepers were dilatory: in a little time he saw a faint light; but after watching hour after hour, there was no appearance of the flash. He then sent a midshipman up the rigging, telling him to report to him as soon as he got into the flash; but it was not till he had ascended a height of 80 feet from the deck that he did so. Below that height there was only the dim light which came through the lower prisms. Most fortunately the flash reached the horizon; but for many miles from the base of the lighthouse there was no flash to be seen, except at a considerable height above the sea. He afterwards visited the lighthouse, and ascer-

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adjustment of flames and apparatus at Grisnez and at South Foreland. See also "Explanation of the Drawings," p. 226, and the drawings in vol. i. For the general subject, see the Report and Appendices. The Commission proved that "the dioptric apparatus in the United Kingdom was not always arranged so as to be turned to the best advantage: that the amount of light produced was often deficient: that the optical apparatus was often so adjusted as to waste a great part of the light produced." But since 1861 it is understood that many of these defects have been remedied, by the use of improved lamps, and by the readjustment of the optical apparatus. The South Foreland dioptric light was visited and hurriedly re-examined by Mr. Campbell, Sept. 15, 1866, and the sea horizon was then seen in the proper direction through all parts of the apparatus, which were tested by looking through the glass. Mr. Campbell said his information was out of date, because it relates to a state of things which appears no longer to exist, at least at the South Foreland.—J. F. C.

tained the cause, with which he had already become familiar in previous inspections of other lighthouses. He found that the instructions given to the keeper were to keep the lighthouse thoroughly clean and to burn as little oil as possible. The keeper was a very intelligent man, and Admiral Ryder learnt a good deal from him as to the smaller details of lighthouse management in the tropics; but the keeper evidently prided himself on his economy of oil. There was a paltry little flame, of not more than an inch and a half in height, and the best part of the flame was at a considerable distance below the foci of the lenses. This was the reason that there was no flash visible from the deck of any vessel, except at a considerable distance. A vessel in thick weather going within that range would be in danger of getting on the rocks. He mentioned this circumstance to the Lighthouse Commissioners, consisting of the naval Commander-in-Chief, Sir A. Milne, the Superintendent of the Dockyard, and others, for it appeared to have escaped their notice. The lamp was not a pressure lamp, but the ordinary lamp used by the Trinity House in England previous to the issue of the report of the Royal Commission. There was no means of rectifying the evil except by raising the lamp, and directing the keeper to burn as much oil as possible. This was the only thing that could be done, as the purchase of a pressure lamp was objected to. The lamp was accordingly raised $\frac{3}{8}$ths of an inch, and the next night after that was done a splendid flash could be seen from the decks of all the vessels in the basin at 4 miles' distance. Thus this light, which no doubt cost between 3,000*l.* and 4,000*l.*, had been worked, up to the period of his visit, in a manner which made it almost useless as a danger-signal at the time when it was most required. He subsequently went to Halifax, another most important port, to which large-class steamers were running daily. At the entrance to the harbour there was a dangerous rock, on which a lighthouse was placed. He made a point of visiting all the lighthouses he could, and accordingly went to this lighthouse, in company with the Commander-in-Chief of the station, Sir A Milne, and there he found the following to be the condition of this most important lighthouse. He found a dirty table, on which were placed nine very poor Argand lamps. There was no arrangement whatever for securing that the rays should be directed in any particular direction, and the lamps were so placed as to leave dead angles. The reflectors were more than fifty years old, and as dull as pewter. He noticed that a small chip of wood was placed underneath the front of each lamp. He asked what the object was in using those pieces of wood, and the reply of the keeper was, that they were used by his father before him, to prevent the oil from flowing over too much; those chips of wood had probably been in use for the last forty years for the same purpose. He measured the angle of error introduced by those pieces of wood, and found that it was from 3 to 5 degrees; so that all the brightest rays had for years been thrown to the stars. He wrote to the Governor of the Province and to the Board of Trade on the subject, and suggested that there ought to be at least a second-class dioptric light at that spot; but he believed the result was nine new Argands or nine new reflectors were substituted, and the pieces of wood, he had no doubt, were still used in the same manner as they always had been. If that was the state of two most important lighthouses in English colonies at no great distance from England, and which were constantly visited by men-of-war and merchant-ships, what was likely to be the state of those situated at great distances on the other side of the world? There was but one remedy, viz., that one or more inspectors of lighthouses should, after being thoroughly instructed in every branch of lighthouse management and adjustment, visit periodically all colonial lighthouses, say triennially, and report in detail to the colonial authorities and the Board of Trade.

Mr. Chance, in reply upon the discussion, said regret had been expressed that certain subjects had not been treated in the Paper. In the few observations he had made after the reading of the Paper, he explained that its limits precluded him from touching upon particular subjects in a manner to do justice to them. He confessed that it had not occurred to him to refer to the constitution of the glass, to which Mr. Siemens had alluded; not because there was any secret connected with it, but because it was a chemical subject; and also because any kind of colourless, transparent glass might be taken to produce the effects required in lighthouse apparatus. The glass used by him was of the ordinary kind—not that called flint glass, which had the higher refracting power. The glass ordinarily used was of low refracting power; its index being under 1·52. Professor Airy regretted that the mechanical portion of the subject had not been more fully treated. That constituted by itself a large division, and could not be introduced into the Paper; but the particular point to which the Astronomer-Royal alluded had been purposely omitted. The Astronomer-Royal referred to the Start Lighthouse, concerning which he expressed the suspicion, after having examined the optical apparatus, that the convex surfaces of the lenses were spherical round one common centre, instead of being annular (except at the central disc), generated round a common horizontal axis, but not having their respective centres of curvature in that axis. He had reason to believe from communications which he had received that such was the case; but considering that this apparatus was constructed about 1836, and looking to the correctness of shape to which the annular lens had been brought by Augustin Fresnel as early as 1822, it was not for him to record how, after so long an interval, such backwardness of execution was still existing in this country. He would, however, suggest, by way of explanation with regard to this matter, that this was nearly the first—if not the first—attempt, in this country to produce the Fresnel lens. It was made at the works of Messrs. Cookson, at Newcastle-upon-Tyne, and what this firm then accomplished was exceedingly creditable to them. His object, however, had been not to record failures, but rather the successive steps of progress, whether in this country or abroad. He was very glad indeed to have had the help of Mr. Campbell and of Admiral Ryder in the explanation of what must have struck all who heard it as a discrepancy between theory and practice. The theory was, that the dioptric light was superior to the catoptric one; whereas it had been asserted that at the South Foreland the former gave no better light than the latter. The reasons assigned by Mr. Campbell and Admiral Ryder were facts to which he could bear witness; and he would add a few words to their remarks, as he himself had inspected the dioptric light at the South Foreland. The refracting portion of the apparatus, as was stated by Mr. Douglass, was constructed at an early date, indeed not long after the first introduction of the cylindrical refractor; it ought not, therefore, to cause surprise that it should be of an inferior description. This, the middle portion of the whole apparatus, as he mentioned in the Paper, constituted in its illuminating value seven-tenths of the whole apparatus; and, further, the single central zone of the refracting portion was equal to half of it in illuminating effect; that was, to seven-twentieths of the whole structure. Now, in the case of the South Foreland light, the refracting portion, especially the central zone, was very inferior, and had never been reconstructed, because it required to be removed for that purpose. Nearly every part of it was doing what formerly characterized the upper and lower divisions of the apparatus; that was, sending valuable light to the sky or to the foot of the tower. Looking at these facts, added to what had been stated by Mr. Campbell and Admiral Ryder, no one would be astonished at finding that the catoptric light, brought to a state of perfection, was equal to a dioptric instrument, which, besides not being provided with an adequate flame, was of a very inferior kind in regard to shape and adjustment; for if the imperfections of the lenses, and the original wrong adjustment of the reflecting parts above and below were considered, it seemed scarcely possible to produce worse effects than what were experienced: indeed, when the light was sent over a large vertical angle, in consequence of imperfection of shape, it did not matter much how the parts were adjusted. The sea would in any case receive only a small portion of the large vertical dispersion. He hoped that he had disposed of the reasons why the dioptric system had not realized in certain lights the merits assigned to it theoretically.

Those who had joined in the discussion had mainly alluded to the rivalry which existed between the dioptric and the catoptric systems; but it had not been his intention to refer to the latter plan in any manner disparagingly. By catoptric apparatus was meant the system of condensing a portion of the luminous sphere of light proceeding from the central source by means of metallic paraboloidal reflectors. He had now to allude to certain proposals which had been made by Colonel Smith. There were two points: one was a suggestion that the metallic reflector should be formed by the revolution of the back portion of a parabola round the vertical axis, which passed through its focus, so as to produce a fixed light over 180°. Now, this might be considered to be the inferior one of the two methods that could be adopted for this purpose. The other method was to cause the anterior portion of the parabola to revolve round its parameter; this being the most effectively illuminating part of the parabola, in consequence of the greater lengths of the focal distances, and the freedom from interception by the burner of reflected light. It was indeed the plan which Bordier Marcet originally devised, and which, to his great disappointment, was superseded so soon by Fresnel's dioptric system of fixed lights.

Another proposal had been made by Colonel Smith. It was for the purpose of producing that which was considered by some a desideratum—namely, to increase the horizontal divergence in the lenticular system. Fresnel proposed, in certain cases, to produce lenticular action by placing outside fixed light vertical refractors, which caused the horizontal condensation. Colonel Smith proposed to fix these vertical refractors within the fixed apparatus, so as still to produce horizontal condensation, but placed so near to the flame that the angle subtended by it at the internal refractors should give the desired angle of horizontal divergence. Now, if this arrangement were adopted, the internal refractors, which were intended to condense laterally, must be cylindrical over the flame, and not vertical; and this cylindrical structure must be cut by two meridian planes, a work which would be troublesome, certainly, but not impracticable. But if the twofold system were adopted, there would be no difficulty in placing outside the fixed light, as Fresnel devised, vertical refractors, which should give by their sections any amount of divergence or convergence. It was not necessary for that purpose to have recourse to an internal structure, which, in addition to the difficulty of its execution, would interpose a most inconvenient obstacle in the service of the lamp. But, in truth, neither of these methods was wanted, whether inside or outside. The desired effect was easily produced by means of a single optical agent, as had been proposed by Mr. Thomas Stevenson. He gave simply to the inner surface of the lens a cylindrical curvature, so that the horizontal section of the inner surface would be a circular arc. By this method horizontal divergence, to any required extent, could be given to a beam of rays, which were before parallel, and at the same time the large proportion of light which would be wasted in its passage through a second optical agent would be saved.

He would now refer to the subject mentioned by Mr. Siemens, who, in alluding to the relative merits of the metallic reflector and the dioptric light, and especially in reference to the two lights at the South Foreland, had made a distinction between the effects of quantity of light and intensity of light. Now, while the source of light remained the same, there could be no difference in the intrinsic nature of the effect produced, and the question of quantity and intensity in relation to each other was simply this:—intensity signified the quantity of light received upon a given unit of surface. If, however, the electric spark were used, it might be an open question, how far the undulations connected with that kind of light might so far differ from those of light caused by the combustion of oil or gas, as to introduce special considerations relating to retardation, or diminution of power, in going through the atmosphere. This was a question quite admissible into the subject; but when discussing the relative effects of the dioptric light and of metallic reflectors, while the source of light remained the same in both cases, the only questions to be dealt with were the loss of light caused by the instrument itself, as ascertained by experiment, and its dispersion in conical beams according to purely geometrical reasoning.

There seemed to be an impression, that the metallic reflectors afforded what was termed a body of light, as distinguished from the effect of dioptric instruments. Now, in a large apparatus such as a 45° segment of a first order revolving dioptric light, the central source of light must not alone be considered, for every part of the apparatus was virtually a radiant point. He was not, however, admitting that size was necessarily an essential element, because, when an observer went to a certain distance, the whole apparatus appeared as a mere point. The effect depended upon the actual quantity of light which the eye received; hence the main points to be considered were—first, the original source of light; then the loss of light caused by the apparatus itself; and finally, the wasteful dilution of light in proportion to the extent of divergence permitted over and above what was wanted for practical purposes. He had seen it stated in the treatises even of scientific writers, that actual size was important in the apparatus, in order that it might subtend a large angle on the sea itself, whereas the whole geometrical portion of the subject related simply to angles within the apparatus, and not to its linear dimensions, as it concerned external angles. He had only referred from memory to what had been remarked during the discussion, but he trusted that he had alluded to most of the points which had been brought forward. There was one, however, which was mentioned by Captain Arrow in regard to revolving catoptric lights; and certainly, if the catoptric system admitted of comparison with the dioptric one, it was in the case of revolving lights. Captain Arrow had specially alluded to the catoptric revolving light at Beachy Head; but Mr. Chance was not aware that, on the south coast of England, there existed any revolving light on the Fresnel system which, by its position, could be viewed in comparison with Beachy Head Light. And he would remark, in regard to the lights on the opposite coast, that neither the Cap Grisnez Light, nor the Calais one, were specimens of the most powerful kind of dioptric revolving lights. He would remind those who knew anything about the light at Beachy Head, that it had the great advantage of not showing a flash more frequently than every two minutes, whereas in dioptric revolving lights generally the greatest interval between the flashes was only one minute. He would rather, in these questions, rely upon accurate observation. There was, indeed, wanting in this country some institution where such points could be decided by actual photometrical experiments, so as to do away with mere conjectures. What was the observation of any mariner worth, when he said that one light gave a powerful effect and another a weak effect, and at the same time gave no particulars concerning the state of the atmosphere? There was nothing so difficult as photometry. The men who handled it under the best circumstances found it difficult to come to conclusions. One rule was indispensable —observations should be simultaneous. It might, indeed, be considered impossible, after looking at any particular light, to remember its intensity. He attached no value to general remarks such as that one light was good and another light bad. It was necessary to appeal to scientific tests, and at present that method was pursued in France alone. The French had carried on from year to year the most systematic course of experiments upon the metallic reflectors and the Fresnel apparatus. Granting that their reflectors were not so good as the English, it was easy to make a deduction for any such difference. What had the most recent experiments proved? That a fixed light of the second order gave an effect equal to that of sixty reflectors of about the ordinary English type. That calculation was not based upon measuring only the brightest part of the beam, but upon the actual quantity of light sent out in a horizontal plane. Take, for instance, a metallic reflector with a horizontal divergence of 15°; the intensity of light relatively to a given unit was measured at each degree, or at each half degree, whence was calculated the total quantity of light contained in the whole angle. In the same manner the intensity of light from the different kinds of Fresnel apparatus was ascertained. It was a matter of simple calculation to compare the quantity of light found in each instrument. The experiments of the French optical Engineers gave results from which it followed that a first order apparatus such as that represented, could be equalled in actual quantity of light in a horizontal plane only by one hundred and eight reflectors of the English type, and by that result he must abide until it was shown to be fallacious by other experiments conducted as scientifically as those made by the French. He wanted to impress upon all that he was merely taking the quantity of light, without regard to its distribution. If this discussion should lead to nothing else than to the institution in this country of some central establishment connected with lighthouse illumination, similar to that at Paris, he felt it would have done great good. Until, however, trustworthy experiments were made in this country, the results obtained in France must be accepted.

Mr. Redman communicated, through the Secretary, the following extracts from an article he had published many years ago on the subject under discussion :—[1]

"The Trinity House have from time to time paid considerable attention to the subject of the different methods of producing light with the greatest effect, and experiments have been made by the Corporation at various periods : in 1827, some were made on the power and brilliancy of Sir David Brewster's lens, the result of which was an impression at that period, that from the small divergence of the light exhibited by Dr. Brewster, it was not so applicable to the purposes of Lighthouses as the common method of Argand burners and reflectors. Various experiments have likewise been made by the Trinity House to test the power of the Drummond light, which for the strength of its flame is probably unequalled, but it has not hitherto been practically made use of on account of its complexity. . . . .

"A Committee of the Elder Brethren have visited the French light of Cordovan. . . . . Experiments have likewise been made at the Trinity House, on the strength and intensity of oil gas ; this gas is however more expensive than spermaceti oil, and has only been used for one or two local harbour-lights. Coal gas from the large quantity of fuel required for its production, and the consequent cost of its transport, and also the uncertainty entertained as to the safety of the light, has not been adopted except in a few harbour lights, and even then in some cases has been abandoned. The same remark may be made of the Bude light as of the Drummond, viz., that if the present objections to its adoption are once overcome, it is likely to become a powerful auxiliary to the present system.

"In former years, and indeed until very recently, beacon-lights were of an uncertain and flickering nature, produced by open fires exhibited from the summits of towers, either by the combustion of wood or coal. Many of the existing towers were formerly lighted in this way, until the mode was abandoned for that of a more stable and certain character. Candles were the next improvement, which we find Smeaton introducing at the Eddystone Lighthouse; and the fact of his recommending them in preference to oil-lamps, shows how little attention at that period had been paid to the subject. The light produced by candles must have been of a faint and varying character, and must have required constant attention in snuffing the wicks; oil had, however, been introduced into Lighthouses previous to the erection of the Eddystone.

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[1] *Vide* "Remarks on the Lighthouse System of Great Britain : including a tabular description of the principal English Lights." By J. B. Redman. [Tract 8vo., vol. lxxii.] London, 1843.

"The introduction of Argand oil-lamps and reflectors was a great improvement, giving a steadier, more certain, and more brilliant light, besides affording facilities for the introduction of distinctions between lights, by making use of revolving frames. These lamps were first used about 1780, at the celebrated French light of Cordovan, and soon after by the Trinity Corporation. In Scotland reflectors were introduced in lieu of coal-lights in 1786, and about the same period in Ireland; and the plan shortly became general.

"It is called the Catoptric system, from the fact that the light from the lamps is reflected and thrown forwards from the plated surfaces of the reflectors, which possess the property of reflecting cylindric beams of light in the direction of their axis, and parallel to the horizon. It results from the figure, that although the reflectors are closely arranged on the supporting frame, there are blank spaces between the beams of light, which are only rendered luminous by aberration and divergence. This is of less consequence in a revolving than in a fixed light, and it is the principal fault of this system. To overcome this objection glass lenses were first used in lighthouses, on the south coast of England, about forty years back. In this case the light is refracted instead of reflected, and projected forward in a horizontal beam, the rays of light being refracted or bent from their natural divergent course, and collected into a luminous belt. These lenses were, however, soon abandoned; for, from their imperfect form, and the fact that the lens was formed of one piece of glass, it was of great thickness, and absorbed a large amount of light, and was thus a less perfect instrument than the reflector. To obviate this objection Buffon proposed the grinding of the lens into steps, or concentric rings; but the difficulty of forming a solid piece of glass to the requisite form, prevented the formation of more than a few specimens of this lens. Condorcet first proposed, in 1788, the constructing, in separate pieces, or as it has been termed building of these lenses. Sir David Brewster also appears to have done the same thing in 1811; and in 1822, Fresnel proposed the same method, and, as it is generally supposed, without any previous knowledge of the two former propositions; and to him belongs the merit of first applying these lenses to the practical purposes of Lighthouses.

"The term 'annular' has been applied to them from their resemblance to the annular rings of timber. The great advantage obtained from this mode of building lenses is the ease with which a perfect figure is obtained for each zone, and the forming of a larger lens than could be procured from a solid piece of glass, and the fact that it is also materially lessened in thickness. The dioptric light is produced by a single powerful lamp placed in the centre of a frame supporting the lenses, composed of from one to four circular concentric wicks, according to the power of lamp required. These lamps, from the more rapid combustion produced, are supplied more quickly with oil than in the ordinary lamp, by its being forced up and made constantly to overflow the wick, and a very tall chimney-glass is made use of to insure a rapid draught for so large a flame. The divergence of light from the annular lens is much less than from the parabolic reflector. These larger annular lenses are used for revolving lights, horizontal prisms of the same section being made use of for a fixed light, producing a belt of light equally brilliant in every point of the horizon. A compound light, termed Cata-dioptric, was also introduced by Fresnel, and is, as the name implies, a combination of the two former systems, being produced as well by reflection as refraction, by means of a series of mirrors arranged in circles above and below the frame supporting the lenses, their diameters decreasing as their distances increase from the focal centre. Cata-dioptric prisms are also used for this purpose; they absorb a less quantity of light, and their property is from their optical form to project horizontal rays by total reflection, from the diverging rays falling on their inner surfaces.

"The great advantage of the French lights, as the above are generally called, is the increased brilliancy and equality of effect in all azimuths. They are divided into four orders or powers of light, which are regulated by the relative diameter of frames and number of wicks to the lamp: in those of the first order the interior radius or focal distance is three feet, and the lamp has four wicks; in the others the radii proportionately decrease, and the lamp by one wick to each order. This classification is not intended as a distinction, but to denote the power and range of lights according to the locality and the distance that they are required to be seen. From the experiments that were made in 1832 and 1833 by the Commissioners of Northern Lights, it appears that the light from one of the large annular lenses used for revolving lights of the first order, was equal to eight of the large reflectors used upon the Scotch coast, and the consumption of oil seven to twelve in favour of the French plan. In the introduction of the French lights into the United Kingdom, the following facts are worthy of notice,—that although the French light is superior in some respects to that used in England, there are circumstances which are materially in favour of the latter plan. The superiority of the Dioptric over the Catoptric consists in diffusing over the horizon an uniform belt of light for fixed lights, by which means they may be observed at equal distances from every point of the horizon, which cannot be obtained by any practical arrangement of parabolic reflectors, nor is the characteristic appearance of the fixed light changed by this method; in the revolving light, however, it is worthy of attention, that from the less divergence of the light in the dioptric plan, the characteristic appearance of the revolving light so well known is materially altered, and the management of the mechanical lamp is rather uncertain—the French revolving lights from this cause having at times been extinguished. In stationary lights, where the whole circle is not illuminated, this objection does not hold good, as the fountain lamp can then be introduced; and even if the mechanical lamp is used, from the greater simplicity of construction the lamp is of more easy access, and the mischief sooner remedied; but both in the revolving and the fixed light there is this objection to the single lamp, that in the event of its going out the navigation is completely deprived of the light; whereas the extinction of a lamp in the Catoptric light involves only a lesser evil, viz., the reduction of brilliancy in the light. There is another peculiarity attendant on the French light; it is this, that from the fact that there is so little divergence of light, the atmosphere below as well as above the belt of light is completely obscure, and when the light is of a local nature it might be approached so close as to be completely obscured. Notwithstanding these objections, however, the French light has been introduced in many instances in Great Britain, and is likewise being adopted in other countries."

May 14, 1867.

JOHN FOWLER, President,
in the Chair.

The discussion upon the Paper No. 1,180, "On Optical Apparatus used in Lighthouses," was continued throughout the evening, to the exclusion of any other subject.

May 21, 1867.

JOHN FOWLER, President,
in the Chair.

The following Candidates were balloted for, and duly elected:—John Daglish, George Baker Forster, Charles Hawksley, William Wilson Hulse, Thomas Grainge Hurst, Robert Valentine John Knight, Dr. Manoel Buarque de Macedo, and Dr. Francisco Pereira Passos, as Members; Wilfrid Airy, Henry John Card Anderson, Imrie Bell, Isaac Lowthian Bell, Edward James Castle, Lieut., R.E., James Timmins Chance, M.A., William Fothergill Cooke, Samuel Thomas Cooper, Charles Baxter Cousens, Henry Halford Coventry, William Grey Ferrar, John Parson, Joseph Potts, Thomas Prosser, and John Barnes Sparks, Lieut., B.S.C., as Associates.

No. 1,183.—"Experiments on the Removal of Organic and Inorganic Substances in Water." By Edward Byrne, M. Inst. C.E.

As an abundant supply of pure water is now universally acknowledged to be most essential for the preservation of health, it is a subject which naturally attracts increased attention. Whilst new sources of supply for the metropolis are still under discussion, and extensive works are being carried out to provide the inhabitants of Dublin, and elsewhere, with good water in abundance, the results of some experiments, on the removal of the organic and inorganic substances in water, recently made by the Author in the laboratory of the Dublin Government School of Science, may not be without interest to the Members of the Institution.

Although these experimental results can only be considered as a slight contribution to a great subject, still, they will not perhaps be deemed without value; for as they were not undertaken to support any theory, and as, in a search after truth, the utmost care and attention were observed to obtain correct results, they will, it is believed, be found perfectly reliable.

Many are the substances spoken of as having a purifying effect on water; but, of all these, charcoal (especially animal charcoal) has been considered the most efficacious.

In works which treat on spring and river waters the assertion is constantly made, that both vegetable and animal charcoal remove the organic and inorganic substances met with in waters. For instance, in Dr. Parkes' Hygiene, page 38, it is stated that, "one part of animal charcoal purified 136 times its weight of any impure water; and 1 part of vegetable charcoal 116 times (Gaultier de Claubry). But if the water be moderately good, 1 part of charcoal will purify 600 times its own weight of water; or 1 lb. will purify 600 lbs. or 60 gallons." However, no experiments are given to enable one to judge to what extent these statements are true. And it is evident, that charcoal cannot purify the same quantity of different samples of water, which vary from each other in the amount they contain of organic and inorganic substances.

The fact is well known, that charcoal removes some classes of organic substances from their solutions, but there are others which it does not remove. For instance, it removes the colouring matter in raw sugar, but not the sugar itself. It removes, from their solutions, a variety of inorganic substances, such as lime, potash, oxide of lead, ammonia, and other bases. It also removes various salts. Graham states that it even separates iodine from iodide of potassium. And Stenhouse has shown that its action on putrescible substances consists in the rapid process of oxidation of these bodies, depending upon the power which the charcoal possesses of condensing oxygen. The question therefore arises,—Is the organic matter, which occurs in water, removed by charcoal, and if so, is it removed unchanged, or is it oxidized like putrescible bodies? Moreover, is either the organic or inorganic matter, after being once removed from the water by charcoal, given up again on the further percolation of water through its pores? These questions seemed important to answer; but first it appeared necessary to ascertain if possible, whether water, uncontaminated by either decomposing animal or sewage matter, but containing dissolved vegetable matter, would contain any nitrogenous bodies, for this important question has been very obscurely dealt with, in treatises on water, except in Dr. Parkes' Hygiene, page 12, where it is stated: "it seems probable that a nitrogenous vegetable matter is not uncommon in waters."

In order to ascertain whether vegetable nitrogenous, as well as vegetable non-nitrogenous matter, is dissolved by waters, a quantity of bog water was procured on which to experiment. This water, which was of a yellowish-brown colour, was taken from a bog hole on the Wicklow side of the County Dublin mountains, between Killakee and Loughbray, in an unfrequented locality, and under circumstances that precluded the possibility of its containing any animal or sewage matter.

EXAMINATION OF THE BOG WATER.

According to Dr. Clark's method, its hardness was found to be 1·20 degree, that of distilled water amounting to about ½ a degree.

The ascertained amount of non-volatile organic and inorganic matter in a gallon, was as follows:—

Organic matter . .	3·02	grains
Inorganic matter . .	1·26	„
Total solid matter . .	4·28	„

It was found, by the aid of a standard solution of permanganate of potash, that the organic matter, amounting to 3·02 grains, required 0·8831 grain of oxygen to oxidize it; the water, upon oxidation, becoming perfectly colourless.

This bog water contained a trace of ammonia; but neither nitrates nor nitrites.

In order to decide whether any nitrogenous matter was present or not in the water, five gallons of it were evaporated to dryness; and the nitrogen, in the solid matters obtained from that quantity, was determined by a modification of Will and Varrentrapp's method. As much as 0·84 grain of nitrogen, that is (dividing by 5) 0·17 grain in a gallon was yielded; proving that in bog water, at least, vegetable nitrogenous matter is present.

Whether on the oxidation of the non-nitrogenous organic substances in the bog water by exposure to the air, the nitrogenous matter remains unoxidized and in solution, or whether it undergoes, like the non-nitrogenous matter, oxidation, the Author is not prepared to state; but he found that nitric acid was formed, when the organic matter was oxidized by permanganate of potash.

That it is possible for nitrogenous organic matter of vegetable origin to exist in water in an innocuous state, may be fairly concluded from the fact, that much soluble nitrogenous matter is found in beer, especially in porter; for, according to the analyses made by Jackson and Wonfor, there is, in a gallon of Guinness's Dublin porter, nitrogen corresponding to 552 grains of albumen.[1]

From the foregoing experiments on bog water, it is manifest that vegetable nitrogenous organic matter can be present in waters; and as this, under the same conditions, will no doubt be decomposed, like animal nitrogenous matter, into ammonia and nitric acid, the conclusions come to by Dr. Frankland, with regard to the London waters,[2] that after deducting the nitrogen corresponding to the nitrates and nitrites of the rain water, any remaining nitrogen must be due to the infiltration of sewage matter, can scarcely apply in all cases.

As putrefactive nitrogenous matter is the most hurtful of all the substances that can be present in water, it must be regretted that, so defective are the methods for estimating organic matter in water by chemical means, no distinction can be made between the nitrogenous organic matter which exists in a putrefactive, and that which exists in a non-putrefactive state.

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[1] See "Journal of the Royal Dublin Society," vol. iii., Nos. 20 and 21, p. 169.

[2] See "Chemical News" of 5th April, 1867, pages 170 and 171.

Examination of the Removing Power of Animal and Vegetable Charcoal.

The water selected for this purpose was pumped up from a garden well at the back of one of the principal streets in Dublin. It was of an agreeable taste; and, in appearance, clear and sparkling.

Four sets of experiments were made with this water.

1st. On animal charcoal.
2nd. On wood charcoal.
3rd. On peat charcoal.
4th. On animal charcoal.

The water in the last case was treated with permanganate of potash, slightly in excess, previous to its passage through the charcoal.

For convenience, the water used in these experiments was conveyed from the well to the laboratory at three different periods; one sample being used for the 1st set of experiments; another sample, for the 2nd and 3rd sets; and a third sample, for the 4th set of experiments. In each case the degrees of hardness were ascertained, as well as the amount of organic and inorganic matter in a gallon; but it was not considered necessary (the total quantities varying so little) to make a complete analysis of more than the 1st sample. This was found to contain no ammoniacal salts, but a very perceptible trace of nitrates.

The following is the amount in grains, of the fixed constituents contained in a gallon:—

Fixed organic constituents, and nitrates .	10·80
Fixed inorganic constituents	88·30
Total solid matter	99·10

The hardness of the water before boiling.	50·50	degrees
The hardness of the water after boiling .	33·00	„
The degrees of hardness lost by boiling .	17·50	„

The amount of oxygen required to oxidize the organic matter contained in a gallon, and which was determined by a standard solution of permanganate of potash, was 0·0116 grain.

The following is the composition of the fixed inorganic constituents in grains:—

Carbonate of lime. . . .	15·144
Sulphate of lime	39·037
Carbonate of iron . . .	trace
Carbonate of magnesia . .	3·260
Chloride of magnesium . .	10·391
Silica	0·740
Alkaline chlorides . . .	19·460

The total amount of solid matter has been already given as 99·10 grains in a gallon; whereas, according to Table 1, the total quantity is 98·832. This trifling difference of 0·268 grain, which is little more than ¼th per cent., arises from the fact, that the estimates were made separately, and in different ways. This water, though containing a considerable amount of organic and inorganic substances, was by no means so impure as the water consumed in many localities. Moreover, owing to the entire absence of matters in suspension, the charcoal was free to act on those merely in solution.

TABLE 1.—COMPOSITION of the WELL WATER. (First Sample.)

	Grains in a Gallon.
Carbonate of lime	15·144
Sulphate of lime	39·037
Carbonate of iron	trace
Carbonate of magnesia . . .	3·260
Chloride of magnesium . . .	10·391
Silica	0·740
Alkaline chlorides	19·460
Organic matter	10·800
Total . .	98·832

The filter[1] employed, and which had been intended for domestic use, was of stoneware, and divided, by two partitions, into three chambers—the uppermost for receiving the water to be filtered, the middle to hold the filtering material, and the lowest for storing the filtered water. The middle chamber, measuring 5½ inches in depth, was packed with charcoal to the depth of 5 inches, resting on a bed of sharp, well washed sand, ½ inch in thickness. Both partitions were perforated with small holes, the upper one being provided with a fine sponge, so as to catch the water as it came in drops from a vessel above, and cause it gradually to enter the filtering material. After diffusing itself regularly in its passage through this, the water finally issued, from the bottom compartment, at the rate of a quart an hour, and fell into a glass gallon measure, as shown in Fig. 1.

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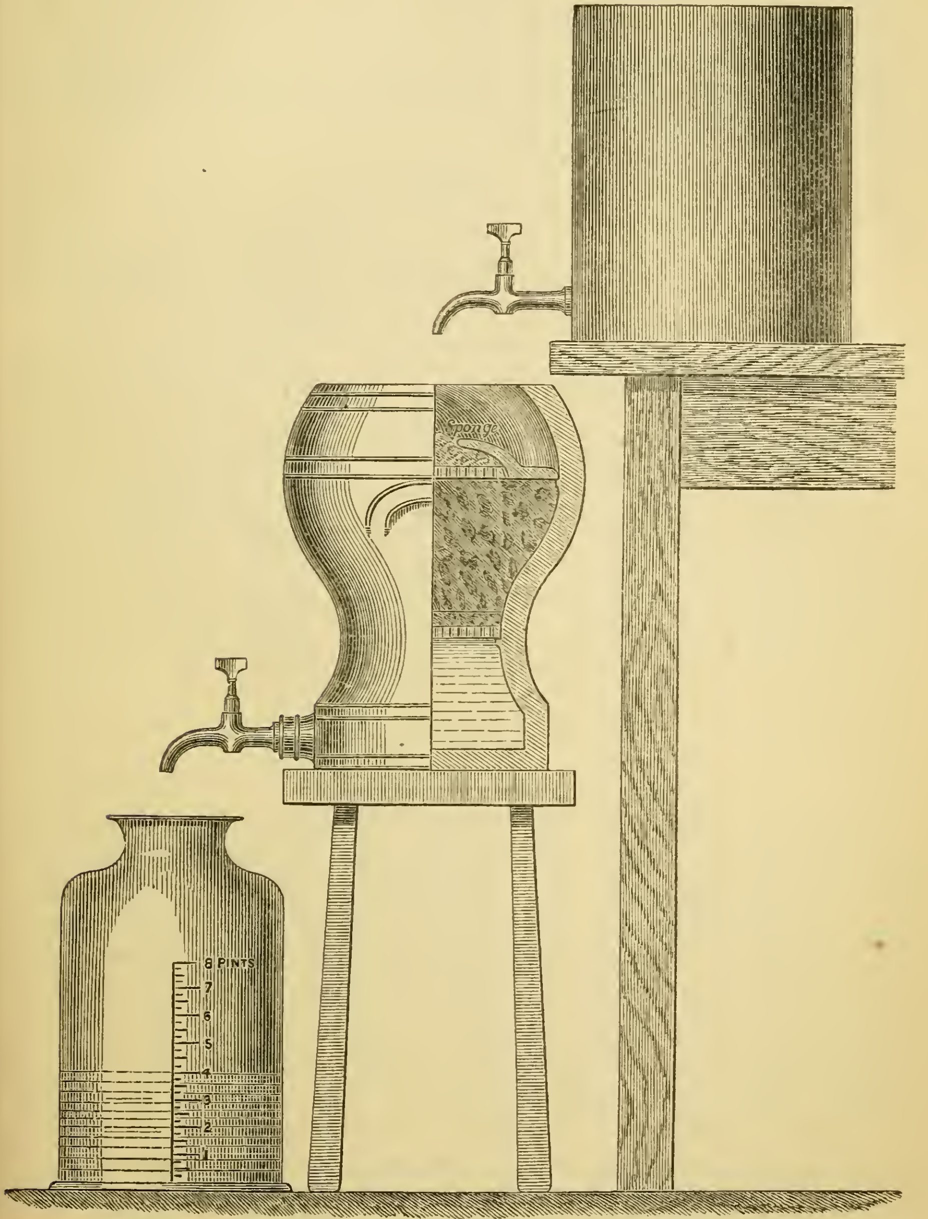
[1] In filtering the water, one gallon (occupying four hours in its percolation) a day was passed through the charcoal. The filtering was discontinued on Sundays, as well as during the night.—E. B.

Fig. 1.



Filtering Apparatus.
Scale 1½ inch to the foot.

Experiments with Animal Charcoal.

The animal charcoal employed was new and freshly burned. It was of the degree of fineness used in sugar refineries, and the quantity of charcoal contained in the filter was $4\frac{3}{4}$ lbs. in weight.

Twelve gallons of water (sample No. 1) were passed through this charcoal, each of which was afterwards examined as to its hardness, both before and after boiling, the differences representing the amount of carbonate of lime and carbonate of magnesia which the charcoal had not removed. The total amount of organic and inorganic matter contained in each gallon of filtered water was also determined, as well as the amount of oxygen required to oxidize the organic matter not absorbed by the charcoal.

Table 2.

Description.		Quantities in Grains Weight found in each Gallon.			Quantities in Grains Weight of Oxygen required to Oxidize the Organic Matter in One Gallon of Water.	Quantities in Grains Weight removed from each Gallon of the Waters.	
		Organic.	Inorganic.	Total.		Organic.	Inorganic.
Original Water.		10·80	88·30	99·10	0·0116	..	..
Water passed through the Filter.	1st gallon .	4·80	35·70	40·50	0·0091	6·00	52·60
	2nd ,, .	6·00	45·00	51·00	0·0091	4·80	43·30
	3rd ,, .	8 40	55·05	63·45	0·0100	2·40	33·25
	4th ,, .	10·65	63·20	73·85	0·0100	0·15	25·10
	5th ,, .	10·70	67·60	78·30	0·0116	0·10	20·70
	6th ,, .	10·71	73·11	83·82	0·0116	0·09	15·19
	7th ,, .	10·85	75·90	86·75	0·0125	..	12·40
	8th ,, .	11·15	76·05	87·20	0·0134	..	12·25
	9th ,, .	11·65	76·40	88·05	0·0183	..	11·90
	10th ,, .	11·73	78·22	89·95	0·0158	..	10·08
	11th ,, .	12·05	79·22	91·27	0·0122	..	9·08
	12th ,, .	12·35	79·50	91·85	0·0122	..	8·80

From an examination of Table 2 it may be seen, that so large a quantity as 52·60 grains of inorganic matter, that is, slightly more than 59½ per cent. of that found in the original water, was removed from the 1st gallon of water that passed through the charcoal. The quantity of inorganic matter removed from each gallon was rapidly less at first, amounting in the 6th gallon to 15·19 grains, or about 17⅕ per cent. of that found in the original water. Afterwards the quantity removed each time decreased less rapidly, amounting in the 12th gallon to 8·80 grains, or nearly 10 per cent. of that found originally. Thus, the quantity of inorganic matter removed, from being more than 59½ per cent. in the 1st gallon, became less than 10 per cent. in the 12th gallon, the actual amount removed being 190·14 grains from the first six gallons and 64·51 grains from the next six gallons of water.

Of the organic matter found in the original water, 6 grains were removed from the 1st gallon, 4·80 grains from the 2nd, 2·40 from the 3rd, and only 0·15 grain from the 4th gallon that passed through the charcoal; that is, on the original quantity, a per centage approximately of 55½, 44½, 22¼, and 1⅓ respectively.

The quantity of organic matter removed from the 6th gallon was only 0·09 grain; that is, less than $\frac{1}{10}$th of a grain, or $\frac{4}{5}$ths per cent. of that found in the original water; and here the power of the charcoal to remove organic matter evidently became exhausted; for immediately afterwards it commenced to part with a portion of what it had previously taken up. For instance, in the 7th gallon there is an increase of 0·05 grain of organic matter, or nearly $\frac{1}{2}$ per cent. on that found in the original water; and, with a steady augmentation, this increase continued, so as to amount to 1·55 grain, or 14$\frac{1}{3}$ per cent. on the original quantity, in the 12th gallon. Thus, of the 13·54 grains of organic matter removed by the charcoal from the first six gallons of water, 4·98 grains were given back to the next six gallons.

The conclusion to be fairly drawn from the foregoing is, that had these experiments been carried a little further, it would have been found that all the organic matter removed in the first instance by the charcoal would have been given back again. Another observation to be made on the information contained in Table 2 is that, judging by the figures in the column headed "Quantity, in grains' weight, of oxygen required to oxidize the organic matter in one gallon of water," the charcoal had no oxidizing effect on the organic matter; and therefore it had not the same effect on the organic matter in this water that, according to Stenhouse, it exerts on putrescent organic substances.

TABLE 3.

DESCRIPTION.		Degrees of Hardness.		
		Before Boiling.	After Boiling.	Lost by Boiling.
Original Water . .		50·50	33·00	17·50
Water passed through the Filter.	1st gallon . .	9·45	9·45	0·00
	2nd ,, . .	18·26	15·15	3·11
	3rd ,, . .	27·47	23·68	3·79
	4th ,, . .	32·89	29·10	3·79
	5th ,, . .	36·37	32·10	4·27
	6th ,, . .	39·53	33·00	6·53
	7th ,, . .	41·85	33·00	8·85
	8th ,, . .	42·17	33·00	9·17
	9th ,, . .	42·87	33·00	9·87
	10th ,, . .	44·85	33·00	11·85
	11th ,, . .	46·55	33·00	13·55
	12th ,, . .	46·55	33·00	13·55

Table 3 shows the degrees of hardness, both before and after boiling, of the original water, as also of that passed through the animal charcoal; the degrees of hardness lost by boiling indicating, in the case of the original water, the amount it contained approximately of the carbonates of lime and of magnesia; and, in the case of the twelve filtered gallons, the amount of the carbonates of lime and of magnesia which the charcoal had not removed. Evidently, from the 1st gallon all the carbonates were removed; from the 2nd gallon a few grains less; and from each succeeding gallon still less—that is, 17·50 grains from the 1st gallon, and only 17·50 less 13·55, or 3·95 grains, from the 11th and 12th gallons. From the first portion of the filtered water, not only were the carbonates of lime and of magnesia removed by the charcoal, but also some of the sulphate of lime, and chloride of magnesium; to estimate the exact amount of each of which was not considered necessary. It is also worthy of remark that, after the 5th gallon, the charcoal evidently ceased to remove any of the sulphate of lime or chloride of magnesium; but, unlike the organic matter, so far as the experiments went, no portion of these salts was given back to the seven gallons of water that were afterwards passed through the charcoal.

EXPERIMENTS WITH WOOD CHARCOAL.

The wood charcoal was packed in the filter exactly as in the case of the animal charcoal; however, being specifically lighter, the quantity employed was slightly less than 2¼ lbs.

TABLE 4.

DESCRIPTION.		Quantities in Grains Weight found in each Gallon.			Degrees of Hardness.
		Organic.	Inorganic.	Total.	
Original Water		12·85	92·90	105·75	55·63
Water passed through Filter.	1st half-gallon	17·75	117·33	135·08	41·55
	2nd ,,	19·08	101·88	120·96	46·57
	2nd gallon	15·36	100·55	115·91	48·30
	3rd ,,	15·35	92·75	108·10	52·31
	4th ,,	13·35	92·50	105·85	54·16
	5th ,,	12·95	92·20	105·15	54·90

By referring to Table 4, it will be seen that, at first, the wood charcoal acted on the water in such a manner as to increase its natural amount, both of organic and inorganic matter. This action ceased, as regards the inorganic matter, at the 3rd gallon; after which, the wood charcoal commenced to have a removing effect; but not until the 5th gallon did the wood charcoal cease to increase the natural amount of organic matter in the water.

The hardness of the water was but slightly affected by the wood charcoal.

So far the wood charcoal had an injurious effect. Had the trial of its action been carried farther, most probably it would have been found, that both organic and inorganic substances are removed by wood charcoal, though to a less extent than by animal charcoal; for, the five gallons of water, passed through the wood charcoal, seemed to have been just sufficient to wash out those substances that were imparted to the water, and which interfered with its removing power. However, as these experiments on wood charcoal, as far as they went, were not attended with satisfactory results, they were discontinued.

EXPERIMENTS WITH PEAT CHARCOAL.

The peat charcoal, which was packed in a filter as already explained, weighed a little less than 2 lbs.

The result of the experiments with peat charcoal is given in Table 5, and as the action of both the wood and peat charcoal was almost identically the same, the remarks made on the former may be considered applicable to the latter.

TABLE 5.

DESCRIPTION.		Quantities in Grains Weight found in each Gallon.			Degrees of Hardness.
		Organic.	Inorganic.	Total.	
Original Water . .		12·85	92·90	105·75	55·63
Water passed through Filter.	1st half-gallon .	13·50	108·08	121·58	64·48
	2nd ,, . .	14·80	115·12	129·92	77·36
	2nd gallon . .	16·65	90·43	107·08	56·00
	3rd ,, . .	14·75	83·25	98·00	52·21
	4th ,, . .	13·45	82·10	95·55	50·19
	5th ,, . .	12·85	81·25	94·10	49·74

EXPERIMENTS ON ANIMAL CHARCOAL WITH SOME OF THE WELL WATER, TREATED WITH PERMANGANATE OF POTASH, SLIGHTLY IN EXCESS, PREVIOUS TO ITS PASSAGE THROUGH THE CHARCOAL.

Twelve gallons of the water, treated with permanganate of potash, were passed through some fresh animal charcoal, the same in quantity, and of the same degree of fineness, as that employed in the first instance. The object aimed at was to try whether, after oxidizing, by means of the permanganate of potash, the organic matter contained in the water, previous to its passage through the charcoal, any organic matter would be subsequently found present or not in the filtered water. The result obtained, as may be seen by referring to Table 6, was, that organic matter was present, and apparently in the same quantity as before the water was treated with permanganate of potash. But whether it was in the same condition or not, the Author is unable to say; for, as before stated, by the aid of chemistry, no positive distinction can be made between the different kinds and states of oxidation of the organic matter in waters.

TABLE 6.

Description.		Quantities in Grains Weight found in each Gallon.			Quantities in Grains Weight of Oxygen required to Oxidize the Organic Matter in One Gallon of Water.	Quantities in Grains Weight removed from each Gallon of the Waters.		Degrees of Hardness.
		Organic.	Inorganic.	Total.		Organic.	Inorganic.	
Original Water.		14·25	94·54	108·79	0·0152	..	..	56·35
Water passed through Filter.	1st gallon	7·66	41·77	49·43	0·0122	6·59	52·77	13·35
	2nd ,,	—	—	—	—	—	—	—
	3rd ,,	—	—	—	—	—	—	—
	4th ,,	10·15	75·72	85·87	0·0366	4·10	18·82	41·16
	5th ,,	—	—	—	—	—	—	—
	6th ,,	—	—	—	—	—	—	—
	7th ,,	—	—	—	—	—	—	—
	8th ,,	14·65	83·20	97·85	0·0427	..	33·34	48·16
	9th ,,	—	—	—	—	—	—	—
	10th ,,	—	—	—	—	—	—	—
	11th ,,	—	—	—	—	—	—	—
	12th ,,	15·35	88·51	103·86	0·0305	—	6·03	49·70

The decolorization of the permanganate of potash by organic matter cannot certainly be deemed a reliable test as to the quantity of organic matter; but, in these experiments, the permanganate was very useful, as a means of determining whether the organic matter contained in the water underwent oxidation or not in its passage through the charcoal.

Of the twelve gallons of water passed through the charcoal in this set of experiments, the 1st, 4th, 8th, and 12th gallons only were subjected to examination. The quantity of inorganic matter removed from these was 52·77, 18·82, 11·34, and 6·03 grains respectively; that is, on the original quantity, a per centage of $55\frac{4}{5}$, $19\frac{9}{10}$, 12, and $6\frac{1}{3}$. Of the organic matter, 6·59 and 4·10 grains respectively; that is, on the original quantity, a per centage of $46\frac{1}{4}$ and $28\frac{3}{4}$, were removed by the charcoal, from the 1st and 4th gallons.

Soon after passing the 4th gallon through, the charcoal manifestly lost its power to remove more organic matter; for, to the 8th gallon it gave up 0·40 grain, and to the 12th gallon 1·10 grain of the organic matter removed in the beginning; that is, on the original quantity, a per centage of $2\frac{4}{5}$ and $7\frac{3}{4}$ respectively.

Not the least useful object gained by these last experiments has been a most unquestionable confirmation of those in the first series; for the quantity of organic and inorganic matter removed, by a given amount of animal charcoal, corresponds very closely in the two sets of experiments.

From the foregoing it has been seen how the power of nearly 5 lbs. of animal charcoal to remove organic matter became completely exhausted by the percolation of six gallons of water ; and that, afterwards, the organic matter removed at first was given back again—a fact sufficient in itself to contradict the general statements made as to its removing power ; to demonstrate how very little indeed can be done by this filtering substance, even on a small scale, towards the purification of water, and to show the delusion under which a large portion of the community has laboured with regard to its efficacy.

As the question of an abundant supply of good water to towns is one that involves a large expenditure of money, it cannot be a matter of surprise that it should be under discussion for a considerable time before the adoption of any particular scheme. Meanwhile, the epidemic which has so recently left these shores may again return. With a view, then, to check its ravages in ever so trifling a degree, it would be worth while to experiment on various materials which are believed to possess the power of removing organic matter ; but to obviate false conclusions, and in order that such experiments may be practically useful, they should be systematic. However, as there appears to the Author, particularly since the completion of his recent experiments, no reasonable hope to expect that, by chemical agency, bad water—especially on a large scale—can ever be purified to the extent required for drinking purposes, he thinks that the public mind should be given more than heretofore to the great question of supply ; and, as people value their lives, they should, above all things, in their choice of a source, not be too much influenced by distance, but be willing to undergo the necessary expense of securing the object of their search, not only in abundance, but in the greatest purity.

It was announced that the Discussion upon this Paper would be taken at the first meeting of the next Session.

May 28, 1867.

The Session was concluded by a CONVERSAZIONE, at which the President received the Members of the Institution, and a numerous circle of distinguished visitors. The rooms were decorated with many choice works of art, and there was also exhibited an interesting collection of mechanical models.

APPENDIX TO VOL. XXVI.

MEMOIRS.

Mr. BENJAMIN HALL BLYTH was born at Edinburgh, on the 14th of July, 1819, and was the son of Mr. Robert Brittain Blyth, an extensive iron and metal merchant, a native of Birmingham. He early showed signs of great power in mental arithmetic. One of these occurred when he was only six years old, when, walking early one morning, he asked his father the exact hour of his birth, and what o'clock it was at the time of his question. He then walked on for a few hundred yards, and turning to his father, said that he had been so many seconds in the world. His father noted the figures, checked the calculation at home, and found that the child, having allowed for the additional days of two leap years, was perfectly correct in the result. He received a good general and classical education at various schools in Edinburgh; always stood high in his class, but showed a special talent for mathematics, solving mentally problems in geometry and algebra, as well as in arithmetic. For various reasons his education was arrested at the too early age of fifteen years, and, in 1834, he became a pupil of Messrs. Grainger and Miller (MM. Inst. C. E.) It was soon perceived that he had made choice of a profession suited to his tastes and acquirements; he rose rapidly in the estimation of Mr. Miller, to whose work he was specially attached, and during the second year of his pupilage he was intrusted with important business. About the year 1841 he received from Mr. Miller the appointment of Resident Engineer on the Kilmarnock branch of the Glasgow and Ayrshire—now the Glasgow and South Western Railway. This line was finished under his superintendence within the specified time, and in a few months after the completion of the works he closed and settled all the Contractor's accounts, receiving from his friends, and from professional men connected with him at that time, many private marks of their regard and esteem for him as a business man.

About the period of his leaving this situation, he was so convinced that Civil Engineering was likely to become a precarious profession, that he tried hard to obtain the appointment of Secretary to the Glasgow and Ayrshire Railway, at a salary of £200 per annum. In this he was disappointed, standing only second in the choice of the Directors; and he often used to allude, in after years, to his good fortune in having by this defeat been prevented from forsaking a profession of which he was so proud, and for which he showed such aptitude.

Soon after this, the busy engineering years of 1844 to 1846 having arrived, he returned to Mr. Miller's office, where he rose to be the principal assistant. He was there intrusted with many important and extensive schemes, including the laying out of the extension of the Glasgow and Ayrshire Railway from Kilmarnock to Carlisle, with numerous less important branches of that Company—the North British, the Direct Northern from London to York, &c. The extent of the work he had to do at this time will be best estimated by the fact that, in November 1845, Mr. Miller, his chief, deposited plans for schemes comprising upwards of 1,500 miles of railway, on most of which Mr. Blyth was engaged in consequence of the high estimation in which he was held and the implicit confidence placed in him.

In the beginning of the year 1850 he commenced business on his own account, his first work being the Slamannan and Bo'ness Branch of the Monkland Railway. In 1852 he was appointed Engineer-in-chief to the Great North of Scotland Railway, then about to be commenced; from that time his business continued to increase so rapidly that in 1854 he admitted his younger brother, Edward Blyth, as a partner. He acted as adviser and Engineer, at various times, to most of the principal Railway Companies in Scotland, including the Caledonian, Great North of Scotland, Glasgow and South Western. Monklands, Scottish Central, Dundee and Perth, Port Patrick, and others. For these Companies he constructed many important lines and branches; but they represented only to a small extent the laborious and extensive employment which he received.

As a Parliamentary Engineer, Mr. Blyth was very extensively employed, and there was rarely an opposed Scotch case in which he was not engaged. He lodged numerous plans for railways which were withdrawn or failed to pass through Parliament, but he was generally successful in his contests. As a witness he was much sought after by English, Welsh, and Irish, as well as Scotch Companies, having acted in that capacity for the Great Western, Great Northern, Midland, and many other Railway Companies.

One of the most eminent living engineers said of him, "I have always looked upon him, in his professional position, as one to be admired and imitated, not only for his ability, but for the honest and straightforward conduct which he always showed as a professional witness, never allowing himself to be swayed from his own opinion; and I believe this was the opinion entertained of him generally by his brother engineers." He was, indeed, peculiarly earnest and upright in all his actions. He possessed the rare quality of avoiding even exaggeration; and if counsel in their questions hit upon a weak point in his case, he would at once admit it. He often said that, apart from this being in accordance with true principle and his oath as a witness, it was good policy, as Committees more readily believed a witness who would admit that his side was not always in the right.

An eminent barrister having one day remarked to him that it must be very difficult for engineers to reconcile to their consciences much of their evidence, he answered, "Not at all; our evidence consists of selected truth given in our proofs; but if, in cross-examination, adverse truth is elicited, I never shrink from it."

He was not only valued by his employers as a witness, but was held in great esteem by his professional brethren and by counsel; many of the latter delighted to examine him in chief, while they had little hope of aiding their cases by his cross-examination. He had a remarkably accurate and acute memory, and while he could cast aside entirely a case which was concluded, any reference to it, years after, seemed to recall all the details vividly and correctly. He took special pleasure, when about to be examined in any case involving numerous figures, in glancing over his notes, so that he could give all his evidence without reference to them when in the witness-box; and often his answers, apparently the result of memory, were in reality mentally worked at the moment so rapidly as to escape detection.

Of late years this extensive professional employment told upon his frame; he frequently complained of fatigue, and constant mental strain was the origin of the disease which sapped his life while yet in its prime. He was thorough in all he did, having a subtle and thoughtful mind, to which new modes of arriving at a result constantly presented themselves, and he had scarcely developed an idea ere another arose. During the last two winters this activity of mind was sorely taxed, as he had numerous schemes to prepare, of plans for Parliament, besides advising continually in matters of policy. He spent restless, wakeful nights, frequently occupying the greater part of them in mentally calculating and designing. On such occasions he would appear early at his office, and with great celerity develop by figures or sketches the ideas he had matured, instructing his assistants to carry them out by detailed plans.

In private life, Mr. Blyth was bright and joyous in hours of happiness, full of sympathy, and ever ready to help in times of sorrow; charitable in his judgment of others, lowly in his estimate of himself, he was wise as a counseller, true and generous as a friend, and tenderly affectionate in all his domestic relations.

This is not the place to enlarge upon such points, but it would not be truthful to omit to remark upon his religious convictions, as governing his business transactions. A relation thus writes of him: "For many years of his life he was guided by strong religious principles; even before these gained the mastery in him, his conduct was marked by the highest honour and integrity; but they opened his heart and hand in a way unknown before, inspiring him with new sympathies, and prompting him to acts of liberality, alike for general philanthropic purposes and for more purely spiritual objects. Recognising himself as only a steward of all that he possessed, he felt it his duty faithfully to apply all his mental powers to his profession, and equally so to honour the Giver of them, by devoting the first-fruits of all his gains to purposes of beneficence and Christian enterprise, dedicating a fixed proportion of all his means, and carefully selecting the objects to which it was to be applied. This systematic beneficence was the secret of a liberality which surprised some, and seemed lavish even to extravagance to others, while he considered it only as a privilege."

Such was the man who has passed away—combining eminence in his branch of the profession, with piety and active benevolence; leaving a memory which will long be honoured, and an example which it would be well to follow.

Although for a considerable time it was evident to his friends that his health was failing, he always cheerfully alluded to a little relaxation in summer as all that he needed; and it was not till the beginning of May, 1866, that he became alarmed by the representations of his medical advisers, and he agreed, at their urgent request, to give up entirely, for a time, all attention to business. He then recognised the precarious tenure by which he held his life, and often alluded to its uncertainty and probable brevity, made all necessary arrangements of his affairs, and lived daily as if each were to be his last.

As usual, he went with his family in August to North Berwick, a watering-place on the east coast, near Edinburgh; there returning vigour and strength raised hopes of his ultimate recovery, only to be suddenly and sadly disappointed. On the evening before his decease (August 21, 1866) he attended a meeting of the managers of the United Presbyterian Church at North Berwick, and made them a liberal offer for the purpose of building a new church, conditionally on certain specified efforts being made by the congregation; he closed his offer by stating that, if not accepted, his health was such as to forbid dependence on its ever being renewed. A few minutes after uttering these words (only having time to receive an unanimous acceptance of his offer) he fell into the arms of his brother, was carried home, and in a few hours passed gently away, in the 48th year of his age, leaving a widow and a large young family to mourn their irreparable loss.

Mr. Blyth joined the Institution of Civil Engineers, as an Associate, in the year 1844, and was transferred to the class of Members in 1851. His residence in Scotland precluded frequent attendance at the meetings; but he was much attached to the Society, and did everything in his power to promote its objects.

MR. FREDERICK BRAITHWAITE, who was born on the 20th of June, 1798, was the fourth son of the late Mr. John Braithwaite, the founder of the well-known manufacturing business in the New Road, London. The early reputation of the firm was to some extent due to the ingenious and successful use of a diving-bell for raising the 'Hartwell' East Indiaman, lost off Bonavista, then the Spanish floating batteries at Gibraltar, and subsequently the sheet anchor of the 'Royal George,' and the whole of the freight of the 'Abergavenny' Indiaman. Frederick Braithwaite was educated at Lord's Grammar School at Tooting, and first became more immediately connected with the profession and with mechanical engineering in 1837, when he succeeded his brother, Mr. John Braithwaite (M. Inst. C.E.), in the conduct of the engine factory, on the occasion of the latter being appointed Engineer-in-Chief to the Eastern Counties Railway; and, in partnership with the Messrs. Milner, he for several years carried on that establishment for the construction of steam engines and machinery, during which he designed and executed several important engineering works.

In the course of a long practice, a great number of wells, which had been sunk by the firm in and around London, remained under its supervision, and thus Mr. Frederick Braithwaite was led to give particular attention to all matters connected with the water-supply of the Metropolis. Among other cases he exemplified his views by a model of a well sunk by him in the year 1841, at Messrs. Reid's brewery,[1] the principal feature of which was a large chamber constructed in the chalk, whence there were driven lateral galleries in various directions to strike the water-bearing fissures, as had been done previously at Messrs. Meux's brewery. On this, as on all similar occasions, he was personally most active, sharing the no slight risks of those employed. At a later period, basing his calculations mainly upon records, kept by his brother, Mr. John Braithwaite, since 1810, of the depths at which the underlying strata were struck, and from daily observations taken under his own direction for a number of years, he prepared an extensive series of sections,[2] showing the actual configuration of the chalk stratum under London, contrary to the well-known theory of the late Dr. Buckland, who conceived the existence of a complete basin. He also exhibited many ingenious diagrams, showing the gradual and steady decline of the water in the deep wells. For a Paper connected with this subject, "On the Infiltration of Salt Water into the Springs of Wells under London and Liverpool,"[1] he received from the Institution of Civil Engineers a Council Premium of books, as well as for a subsequent Paper "On the Rise and Fall of the River Wandle; its Springs, Tributaries, and Pollution."[2] He also contributed a Paper "On the Fatigue and consequent Fracture of Metals,"[3] and in general took an active part in many important discussions.

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. ii. (1842), p. 164.
[2] *Ibid.*, vol. v., p. 478.

In 1848 he went to Lisbon, where he resided for a considerable time, in the endeavour to establish waterworks in that city; and in 1850 he gave evidence before the Board of Health on the supply of water to the Metropolis.[4]

He latterly devoted much of his time to questions concerning the preservation of iron-clad vessels, and became an Associate of the Institution of Naval Architects. He was a man of intelligence and of considerable attainments, and was much esteemed in private circles for his genial temper. He was well read in geology, chemistry, and other natural sciences, and possessed good perceptive faculties.

Mr. Braithwaite joined the Institution of Civil Engineers as an Associate, May 29th, 1838, and was transferred to the class of Members, March 11th, 1845. He served as an Associate of Council in the years 1842 and 1843, and took an active part in bringing about the limitation of the period for holding the office of President of the Institution. He died on the 27th of February, 1865, in the 68th year of his age.

MR. ROBERT DAGLISH was born on the 21st of December, 1779. He settled at Wigan in the year 1804, as Engineer to Lord Balcarras, the father of the present Earl, where he managed for his lordship, the engineering establishment now known as the Haigh Foundry and Brock Mill Forge. He there constructed the Arley colliery engine, and many other pumping, winding, and blast engines, which were celebrated in their day as improved and efficient machines. After some years' experience at these works, Mr. Daglish took the management of the Orrell Colliery, near Wigan, and whilst there he constructed the railway in connection with it. He at once appreciated and applied to this railway the then novel invention of the locomotive steam engine of Mr. John Blenkinsop, of Leeds, in which the power was applied by means of a large cog-wheel working into a rack laid down beside the ordinary rail. By arrangement with the patentee, Mr. Daglish constructed a locomotive of this description in the year 1812, and started it on this railway in 1813. It was known as the "Yorkshire Horse," and was long looked upon with interest by all concerned in the advancement of engineering science. Under Mr. Daglish's management, the Orrell Colliery became a most successful commercial undertaking, and "Orrell coal" in that district has become the equivalent for the "Wallsend" of the north.

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[1] Minutes of Proceedings Inst. C.E., vol. xiv., p. 507.

[2] *Ibid.*, vol. xx., p. 191.

[3] *Ibid.*, vol. xiii., p. 463.

[4] *Vide* "Report on the Supply of Water to the Metropolis." 1850. Appendix No. II., p. 93.

In the year 1825, Mr. Daglish projected, and commenced the survey for, the Bolton and Leigh Railway, in which undertaking, besides the ordinary engineering difficulties, he had to contend against, and eventually to yield to, the wishes of a large landed proprietor, who objected to embankments and cuttings, and insisted upon the line undulating with the natural surface of the land.

Soon after the introduction of the railway system for passenger traffic, the Directors of the London and Birmingham Railway—the largest undertaking of the time—by the offer of a premium of £100, by an advertisement in the columns of "The Times," invited public competition for the best form of parallel rail and pedestals. Mr. Daglish took up the inquiry, and in his tender dated the 3rd of December, 1834, he said: "I am quite sure a velocity of from fifty to sixty miles per hour may be obtained upon a well-constructed railway with greater safety than twenty miles upon any of the present lines yet in operation, not only from their having too light a rail and an ill-constructed pedestal, but also from the mode of fixing them, especially at the joints, which is the great cause of so much deflection and sudden action both vertically and horizontally; so that it is not in the power of man to make a locomotive engine to stand the action they are subject to long together, besides the like effect upon every carriage used upon the line, which can only be attributed to the defective principle of the rails, &c., together with bad workmanship at the commencement." He thus early recognised the difficulty which has at all times occupied the minds of engineers, and of which the fish-plate has now been offered as a partial solution. He also felt the necessity of having the rails of sufficient weight to resist the heavy traffic, which he foresaw must necessarily pass over them; and in the same tender, whilst alluding to the wish of the directors, that the weight of the rail should not exceed fifty pounds per yard, he said—"Allow me to assure you that every public railway will never regret having sufficient strength in the rails at the beginning, and ought not by any means to confine themselves to a pound or two in the yard, in order to make such a valuable work as the one in question as complete and substantial as possible at the commencement." This advertisement was answered by almost every engineer of eminence at that period, and out of no less than seventy-two competitors, the premium was awarded to Mr. Daglish. His prize rail and pedestal or chair became generally adopted. Mr. Daglish did not patent this valuable invention, but generously threw open its use to the public.

For many years—indeed to within a short time of his death—Mr. Daglish enjoyed an extensive practice as a mining and civil engineer, his advice being sought by many of the principal colliery proprietors in Lancashire, Cheshire, and North Wales. He was largely consulted by foreign as well as by English railway companies. The Great North of England Railway; the Boston and Providence Railway; the New York and Haarlem Railway; the Norwich and Worcester Railway (U.S.); and the Bootle Waterworks, are some of the undertakings upon which he was consulted. He was, some years ago, one of the projectors of, and a partner in, the St. Helen's Foundry, which is still carried on by his son and grandson.

The important subject of the ventilation of mines occupied much of his attention, and he invented an improved anemometer for measuring the air passing through colliery workings. In his own neighbourhood his exertions to ameliorate the social and religious condition of those around him are well known, and will be long remembered.

During an active business life of upwards of sixty years, Mr. Daglish secured and retained to the last the respect and esteem of all who had the good fortune to make his acquaintance, and the memory of his life and works will be an incentive and encouragement long felt amongst those who remain behind him.

He was elected a Member of the Institution on the 30th of March, 1830, and died at his residence at Orrell, Lancashire, on the 28th of December, 1865, at the advanced age of eighty-eight years.

Mr. JOHN DINNEN was the eldest son of Mr. Andrew Dinnen, and was born in Liverpool, on the 25th of January, 1808. His father was at that time in business as a brass and iron founder, but he quitted Liverpool a few years subsequently to fill the responsible situation of master-founder and metallurgist of her Majesty's Dockyard at Portsmouth. Mr. John Dinnen received his education under the Rev. John Neave, of the Grammar School, Portsea, and at an early age showed great aptitude for mechanics. After completing his educational studies, he was apprenticed to the late Mr. Simon Goodrich (M. Inst. C. E.) and Mr. William

Kingston, head machinists of her Majesty's Dockyard at Portsmouth, under whose supervision he became a finished draughtsman, and gained a thorough knowledge of the foundry in all its branches. Mr. Goodrich, finding him full of intelligence, employed him in many important duties connected with the then steam arm of the service.

At the early age of twenty years he was appointed Assistant Engineer to H.M.S.S. 'Lightning,' one of the three steam-vessels then comprising her Majesty's steam navy; and in the course of twelve months he became Chief Engineer. Here his superior intelligence soon manifested itself, and the management of marine steam-boilers occupied his close attention. About this time he came under the notice of the late Mr. Joshua Field (Past-President Inst. C.E.), who had his attention called to many points in the working of marine steam-engines and boilers, and who, from Mr. Dinnen's suggestions, effected several improvements in their construction. He continued in the 'Lightning' five years. At the expiration of that time he was appointed Chief Engineer of H.M.S.S. 'African,' employed in carrying the mails between Falmouth, the Ionian Islands, and Egypt. At this period the management of steam boilers was but little understood, and several serious failures had occurred. Vessels frequently returned to England with their boilers full of incrustation and salt; and in many instances, after seven or eight weeks' steaming, on the return of the 'African' to Falmouth, Mr. Dinnen would find the vessel, whose turn it was to take the mail, laid up for repairs on account of injury to her boilers from undue deposit.

This naturally directed his attention more forcibly to the subject, and he wrote a Paper on the "Management of Marine Steam Boilers," which was subsequently published in one of the early editions of "Tredgold on the Steam Engine." His views at that time were so far in advance, that had the Paper been produced at the present day, but little could have been added to it. Mr. Dinnen served nearly six years in the 'African,' and from his marked intelligence he was selected as an assistant to the Chief Engineer of Woolwich Steam Engine Factory, where he continued to serve for about ten years. In 1847 a new class of officers—Inspectors of Machinery Afloat—was instituted in her Majesty's Naval Service, and Mr. Dinnen was appointed Inspector of Machinery Afloat to the late Sir Charles Napier's squadron, then on the Lisbon station, and subsequently to Sir William Martin's squadron, in the same capacity. After this he was appointed Inspector of Machinery at the Admiralty, where he was serving when he met with the melancholy accident which deprived the service of one of the best engineer officers of the Navy.

Mr. Dinnen was well educated. He was a good French scholar, and he spoke Italian fluently. He had the faculty of grappling at once with what he had to do, and his reports were generally written off-hand in so lucid and clear a manner as to require little or no revision. He was a good son, a kind brother, and a faithful friend. As a messmate he was the life of the mess;—being full of anecdote, having travelled and seen so much, and being a close observer, he, no matter what the subject, was ready to enter into it, and to garnish it with amusing jokes, a faculty which made him sought for by every one.

Mr. Dinnen was elected a Member of the Institution of Civil Engineers on the 3rd of April, 1860, and he took a useful part in the discussion of those subjects to which he had devoted attention. His death at the age of fifty-eight years, resulted from concussion of the brain, on the 4th of January, 1866, from having been knocked down by a Hansom cab whilst crossing the road on the previous evening, when leaving the Admiralty, Whitehall, for his private residence.

MR. WILLIAM GRAVATT was born at Gravesend on the 14th of July, 1806. In January, 1822, his father, the late Colonel Gravatt, of the Royal Engineers, Inspector of the Royal Military Academy at Woolwich, wrote to the late Mr. Bryan Donkin:—"My son has been brought up to know that he must trust to his own resources for advancement in life. Throwing as far as I can from me the partiality of a father, I must yet be permitted to say that his habits are peculiarly suited for the attainment of the duties of your noble profession, since, to a good classical education he joins a knowledge of mathematics seldom attained at his years, including the practical parts of perspective, and the drawing of plans, elevations, and sections; he has also a most determined bias to whatever relates to machinery, which last I attribute to his having made the steam and other engines in this arsenal the objects of his contemplation."

At the early age of fifteen years, he already showed an appreciation of delicate, mechanical manipulation, as well as an aptitude for mathematical investigation, for both of which he was in after life very remarkable. Shortly after he was placed with Mr. Donkin, a small wager was laid between young Gravatt and a fellow-apprentice, that the latter could not, in a given time, unwind a given length of string from a stick planted in the ground by walking round it. Gravatt's winning the wager led him to get out a formula for determining the distance walked round the fixed stick, which his friend the late Professor Barlow found to be correct, and remarked that the same thing happened to be then under consideration by the cadets of the Royal Academy.

During his apprenticeship under Mr. Donkin he not only went through the usual routine of mechanical engineering, but he was likewise initiated into those scientific acquirements, the value of which, as essential to the professional status of a Civil Engineer, is now more properly recognized, and of which he never lost sight.

At this time began the intimacy with Mr. Troughton and Dr. Wollaston, which continued until their deaths. Through the former he may be said to have been connected by tradition with the father of English Engineers, as the following anecdote will show. At one of Gravatt's frequent visits to Troughton's back parlour, in Fleet Street, he fell into conversation, during her brother's absence, with Miss Troughton, who described the personages therein admitted. "They all praised her brother," she said, "save one, a short man, with a grey coat;" and for this she was inclined "to eye him askant." He, however, came one day "to bid Edward his last farewell," and, at parting, was observed to be in tears. As soon as he was gone, "Now I know," said the sister to her brother, "why he never praised you. 'Twas because he loved you." This man was John Smeaton.

On leaving the works of Mr. Donkin he was placed under Mr., afterwards Sir Isambard, Brunel, who, before Gravatt's apprenticeship expired, "considered that he was a competent person to attend to the management of 'the shield,' in particular, and, in general, to the machinery belonging to the Thames Tunnel." He frequently remained in the frames all night, and more than once, when Mr. I. K. Brunel was incapacitated by sickness, he stayed on duty for thirty-eight hours, spending those needed for rest at the bedside of his friend.

"On the 5th of March, 1828, silver medals were voted by the Royal Humane Society to Mr. I. K. Brunel and Mr. William Gravatt for having hazarded their own lives to preserve those of their fellow-creatures." To the latter was presented, by the two directors, Mr. R. H. Marten and Mr. R. P. Harris, a gold ring, as "A memorial of the events of the 27th of June, 1827, and of his humane, prompt, and efficient help in rescuing them from a perilous situation" during a survey by boat of the interior of the Thames Tunnel after an irruption of the river.

In 1832 he became a Fellow of the Royal Society and of the Royal Astronomical Society, and in the proceedings of both Societies he took an active interest. "I used to meet," says Professor De Morgan, "two Engineers at the Society, one military and one civil, who were very curious about questions of principle in mathematics. The first was General Colby, the second was Mr. Gravatt, who was quick to see, and, certainly, a mathematician of the higher branches. A long time ago he told me that he had derived so much benefit from my work on the Differential Calculus that he must insist on subscribing, as a testimonial of the instruction he had received, to some charity; and he requested me to name an institution. I thought he was in joke, and laughed; but he convinced me he was in earnest, at which I laughed still more. I refused to name any charity, and, as I thought, the matter ended. But, a few days after, I actually received a subscriber's voting-paper for some orphan asylum, which he requested me to fill up in my own way out of the list of candidates, and forward: which I did. There was a curious eccentricity about the method, but it showed a real sense of benefit received; and a person who could have any right to the feeling from so extensive a work on the higher mathematics as my Differential Calculus, must either have been a person who could flatter himself he understood what he did not—a very unlikely thing in so clear-headed a person—or must have had a true understanding of the contents. And this was the impression I derived from other conversations."

Upon the stoppage of the Thames Tunnel works in 1832, he was, by the recommendation of Mr. Donkin, appointed Engineer to the Calder and Hebble Navigation. Here he commenced his first works, in the shape of several bridges across the river, the arches of which were remarkable for their stability and cheapness; in these he was, perhaps, the first to adopt a principle equivalent to that since successfully applied to suspension bridges, *i.e.*, the combination of local rigidity with the distributed support which the curvature of the flexible chain supplies. Thus, while the general curvature of the chain or arch sustains the dead load in *equilibrio*, the addition of longitudinal stiffness to the structure meets the non-equilibrated stress due to unequal or travelling load.

The rise, in Gravatt's bridges, of necessity small, being in the ratio of 4 to a 100 span, he obtained the help of rigidity by an arrangement analogous rather to that proposed by Mr. Cowper, in which the suspending chain, instead of being flexible, is formed of plate iron, so as to possess in the requisite degree the rigidity of a plate or box-girder, than to that of Roebling or Barlow, in which the local rigidity is supplied by the structure of the roadway. The platform of the roadway was laid throughout, on the back of a pair of arched girders, each of which consisted of several layers of timber bolted together, with hard wood keys interposed between the contiguous layers buried half in each, near the bolts, so as to prevent the surfaces from sliding, when pressure had to be obliquely transmitted from layer to layer. In such a structure the stress due to dead load must be throughout compressional, while that due to travelling load may be resolved into :—1. Compressional stress throughout the length of the girder; and, 2. Bending moment, having a local distribution appropriate to the position of the load, and producing tension along the inner, and compression along the outer surface of the girder, or *vice versâ*, according as the curvature which it tends to induce is of a downward or upward character. Under these combined stresses, as they existed in the Gravatt Bridge, it is only when the travelling load rests on a certain region of either haunch, that actual tensional stress is developed; and then only in the fibres nearer to the inner or outer surface of the girder. Hence, in the first place, in arranging the layers, lengths of which his girders consisted, Mr. Gravatt adopted such dimensions that no break of joint in the inner or outer layers should occur within the region in which actual tensional stress could be developed. Thus, the central part of the girder, forming the region of the neutral axis, and therefore but little affected by the forces which belong to 'bending moment,' is, in virtue of its position, exempt from that excess of compressional strain which its inner and outer surfaces along the haunches are occasionally called on to endure. And hence, in the second place, in putting the work together, Mr. Gravatt so arranged the combinations that central layers should, prior to the loading of the bridge, already experience a considerable amount of compression, and should thus, when in place, relieve the inner and outer surface of some portion of that excess of compression which, under the circumstances, tends occasionally to fall on them. In other words: 1. The stress on the equilibrated arched girder loaded throughout is compressional; 2. When unequally loaded, and the one haunch thereby depressed, the other elevated, the under side of one is a little in tension, and its top a great deal in compression; while the other, or opposite haunch, has its under side much in compression, and its top little in tension; in each case, if the centre line, being almost unaffected, be previously thrown into strong compression in each haunch, then, the excess of tension in either, being thrown on the central part in both, is corrected. Now, Mr. Gravatt, in the act of putting the parts of his cambered girder together, developed a permanent stress on some of them, and, by this means, got out of the material the maximum of work.

By Mr. H. R. Palmer he was employed in examining the country for the original scheme of the London and Dover Railway. During the early period of railway construction he devised a level, which generally bears his name, but which he called the 'dumpy,' and also the level-staff, which is now universally employed, but which is not generally known to have been introduced by him. Those whose experience in levelling has been acquired by help of the compact and simple instruments now universally in use, can hardly conceive the range of the step which was made in advance, when these improved appointments, at once matured to a degree which left little room for improvement, were introduced by Mr.

Gravatt above thirty years ago. Mr. Troughton had, indeed, already substituted for the cumbrous Y level, with its easily disturbed adjustments, an instrument pretty firmly mounted, and having adjustments of a formal character. But the telescope, from its large focal length and relatively small aperture, was wanting in compactness and in light ; and no improvement had yet been made on the old-fashioned staff, with its sliding vane which had to be adjusted to the intersection of the horizontal web, under signal from the observer, by the staff-holder, on whose care in making the adjustment, and in preserving it till the staff had been handled and read by the observer, the veracity of the result materially depended. Mr. Gravatt at once grasped the idea that, with a decent telescope and legibly divided staff, the observer could independently read off his own observations while the staff was in position, and he accordingly originated the character of division now universally in use. And applying to the subject his masterly knowledge of theoretical and practical optics, he so arranged the optical details of the telescope as to obtain, with an object glass of large aperture and short focus, breadth and flatness of field as well as excellent achromatization and definition. For these valuable improvements a Telford Medal was awarded to him by the Institution of Civil Engineers.

He likewise contrived a pocket instrument, which he called a 'nadir,'[1] by means of which, and a common box-sextant, without the aid of an assistant or a level-staff, he carried lines of trial levels through districts where the opposition of land-owners or other obstacles rendered the traversing of the country, save by the highways, impracticable.

After the rejection of the first Great Western Railway Bill, and the passing of that of the Southampton Railway, which latter, together with the Bath and Basing line, he helped to oppose, in 1834, he was employed by the late Mr. Brunel to survey a line of railway from Cardiff to Merthyr Tydfil and Dowlais; and, from the passing of the Great Western Railway Act, he was engaged in designing bridges for the first contracts for that line. In 1835, he was engaged by Mr. Brunel to superintend the parliamentary surveys and to co-operate with him in tracing the line for the Bristol and Exeter Railway. This task exhibited his extraordinary and well-known powers of endurance; the whole work, extending over 75 miles, being begun and ended in about a month.. The ability with which it was carried out was proved by the permanent works being executed throughout within the limit of the 100 yards on either side of the line laid down on the original parliamentary plans; in other words, no further deviation therefrom was called for during the construction of the railway. Whilst carrying out the works of this line, of which he had been appointed the Acting Engineer by Mr. Brunel, he was, on his own account, Engineer to the canalization of the River Parret to Langport, where he constructed a stone bridge; and, at Bristol, he designed and superintended the erection of the St. Phillip's drawbridge, the stone piers of which were built on shore in caissons, then launched and sunk in their places.

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[1] The 'nadir,' where an 'artificial horizon' would be an inconvenient and often inapplicable adjunct to the sextant, gives the means of measuring angles not greatly differing from 90°. It consists of a crossed lens fixed in a tube, about 6 inches long, whose lower end has openings to admit light to a white disc fixed in the solar focus of the lens at the bottom of the tube. With lens and disc this tube is suspended very near its top by gimbals, so that it takes, when at rest, a vertical position; the axis of the lens and disc forming an optic plumb-line. Upon the flat white disc consecutive black circles are drawn, which, being in the solar focus of the lens, an optic pencil passing from them through the lens becomes parallel ; they are then seen through the sextant-telescope focused to a distant object.

The telescope being previously focused to the object aimed at, the index glass of the sextant is held over the stationed 'nadir' and moved till the central dot of the disc reflected from the index-glass coincides with the object: thus, determining the angle in a vertical plane between the observed object and the prolongation of the optic axis of the 'nadir.'

Mr. Waterhouse, from whose description the above is condensed, says: 'I have measured the model of the Calder bridges, and I find the span 200 inches, with a total rise of 4¼ inches: my impression is that the bridge itself is 103 feet span, with 3 feet 9 inches rise.'

During the railway mania of 1845-6, Mr. Gravatt unwisely accepted the post of Engineer to several lines, which turned out mere bubbles, and by whose promoters he was not only left without professional remuneration, but he was saddled with the cost of the surveys and preliminary charges. His position was thereby, not only then, but prospectively, affected; and during the rest of his days he suffered more or less from those severe disappointments.

In 1850 he was, on account of his well-known mathematical and mechanical acquirements, selected to design and to superintend the construction of an Achromatic Telescope, which was to have been the largest that had hitherto been produced. For the object-glass, the refractive-index and dispersive-ratio of the flint and crown-glass parts were determined by a series of careful experiments; and from the data thus acquired the curves of the two lenses were calculated for a combined focal length of 76 feet. The first observation made showed the divisions in Saturn's ring, the slate-coloured ring, and the inner satellites with great distinctness, likewise α and ϵ Lyræ with their minute companions; and the superiority of this object-glass of 24-inches diameter in its command over the faintest points of light was admitted by all observers.

Equilibrated by a weight slung over a round brick tower, 64 feet high by 15 feet diameter, the tube could be placed at any altitude and at any azimuth by the observer himself—a railway carrying a light frame on wheels for its movement round the tower. The ease of manipulation was perfect, and the arrangements for neutralizing the vibration of this tube of 85 feet length produced the utmost steadiness. The late Mr. George Rennie (M. Inst. C.E.), who constructed the tube, declined to remove the blocks upon which it had been built save in the presence of Mr. Gravatt. The amount of flexure which took place, upon the removal of all but the end blocks, was, however, exactly what he had calculated it to be, and no more. Both in the design and in the mechanical execution of this telescope, Mr. Gravatt proved himself not alone worthy of having sat at the feet of Troughton, but also in advance of the times. In more instances than one where he corrected the working formulæ of the leading authorities, the words of his friend Mr. Babbage were confirmed, when he said that—"Gravatt's mathematical capacity was of a high order, and mathematicians were glad to consult him; when they had not done so, it was a great satisfaction to them to find that his opinion coincided with their own." The space-penetrating power of this giant refractor would have extended the boundaries of astronomic science; but, in spite of Lord Spencer's liberal gift of two acres on Wandsworth Common for it to stand upon, the unforeseen pecuniary difficulties of the person for whom it was constructed prevented its entire completion, and the whole is now falling to ruin; its tube, like a rejected toy, lying on the ground, "Jacet ingens littore truncus avulsumque humeris caput et sine nomine corpus." The only existing proof of its work is a photograph, about 9 inches diameter, of the full moon, taken by the Rev. J. B. Reade, on the principal focus, *i.e.*, the focus of the object-glass, on the 6th of September, 1854, in which all the important features in the moon's surface could be discerned. A positive print of this was sent to the International Exposition of Paris in 1855, and received 'Honourable Mention,' with a special notice in the Jurors' Report.

When M. Scheutz, in 1854, brought his 'difference engine' to London, Mr. Gravatt at once took up a subject which had long interested him, and he soon mastered the difficulties of that ingenious machine. In 1855 he undertook the task of explaining to Prince Albert and to the Fellows of the Royal Society its principle and mode of action, and when it was placed in the International Exposition at Paris he continued to illustrate its construction and action; in fact, "Without him," says Mr. Babbage, "we should have had no calculating machine." The instrument, therefore, which was constructed by Messrs. Donkin after the Swedish original, and which is now worked at Somerset House under the direction of the Registrar-General, is due to Mr. Gravatt. Under his direction, specimens of logarithmic and other tables were calculated and printed without the use of types, thereby establishing the faith of the public in its capability of not only performing but also of registering complicated calculations.

He likewise calculated, stereoglyphed, and printed without the use of types, some mountain barometric tables, which, in the form of a *brochure*, were circulated among his friends.[1] Amongst the educated classes of the Chinese, who avoid politics, a taste for the mathematical sciences is very prevalent. The numerous translations of western scientific works attest this fact, and among their reproductions is the reprint by them of Gravatt's "Companion to the Mountain Barometer." For his services rendered to science, in connection with the Swedish difference engine, he was elected Foreign Member of the Royal Academy of Sweden.

Mr. Gravatt joined the Institution of Civil Engineers as an Associate in 1826, and in 1828 he was transferred to the class of Members. He was a constant attendant at the meetings, frequently taking part in the discussions, especially on the occasion of novel, or abstruse questions. He took a prominent part in the discussion on the theory of the jet-propeller, and on the submerging of telegraphic cables.[2]

It is always difficult to put, in an acceptable shape, before an assembly of practical men, a purely mathematical exposition of the fundamental dynamic conditions which underlie the solution of a practical problem; especially when, on the one hand, the elucidation involves intricate and abstract modes of thought, and when, on the other, the theoretical conclusions drawn therefrom must, in their application to practice, be largely modified by obvious, though collateral practical conditions. Now, Mr. Gravatt's manner in discussion, though convincing in private, was not such in public as to master this difficulty so as to do justice to his matter. It was perhaps attributable to these circumstances that, in the controversy which followed the reading of Mr. D. K. Clark's Paper in 1854 on "Ruthven's Propeller,"[3] his view was warmly opposed by several eminent members of the Institution. Nevertheless, the view itself, exhibited in succinct and masterly mathematical language, is (regarded in a purely dynamical light, and taking account of its entire fundamental dynamic conditions) a true solution of the question at that time at issue. If by the labours of more recent investigators, like Dr. Rankine and Mr. Froude, this solution has, on the practical side, been extended, Mr. Gravatt's, so far as it went, not only remains uncontroverted, but has received unqualified confirmation. It is to be found among the pamphlets in the library of the Institution, in "A Letter on Steam Gun-boats of shallow draught and high speed."[4]

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[1] *Vide* Mountain Barometer Tables; calculated and stereoglyphed by Messrs. Scheutz's Calculating Machine No. 2, and printed by machinery. London, 1859.

[2] *Vide* Minutes of Proceedings Inst. C.E., vol. xvii., p. 322.

[3] *Ibid.*, vol. xiii., p. 370.

[4] *Vide* Tracts, 8vo., vol. 153.

His view of the mathematic-mechanics of laying submarine cables was clear and profound, and in himself wholly original; but not absolutely, with regard to others who preceded him in laying down the theory. At the termination of the controversy which took place between himself and the Astronomer-Royal, the latter said,—"Though I partly disagree, I do in great measure agree with you, and am rather surprised that with so little calculation you should have arrived at results of such general accuracy." Mr. Gravatt published in the "Philosophical Magazine" of July, 1858, a further elucidation of the conclusions at which he arrived during the discussion on Messrs. Longridge and Brooks' Paper read at the Institution in February, 1858.[1]

Mr. Gravatt was, in the strictest sense, a remarkable man; peculiar to a degree which may fairly be called extraordinary, even in his defects, especially in those usually associated with want of sense, and implying that want; combined, however, with the possession on most points of the most solid and widely ranging perception and judgment—"common sense," in the strictest meaning of the term. He was like one of those wonderful pieces of work of his friend Troughton, which, for some small blemish or defect, its maker consigned to the limbo of the brass-box to be sold some day as old metal, although fit for all but the most perfect performance of the work for which it had been intended.

Capable of the warmest and most persistent friendship, Mr. Gravatt was also susceptible of strong resentment. This was, for the most part, based on a perception of wrong, as such, and expressed his strong and clear disapprobation of wrong doing. It was also sometimes tinged with personal feelings, and warped by the operation of one or other of those disturbing mental forces which have already been alluded to; and, as a natural consequence, it was at such times often unjust. But if ever he came to see, as he was never unwilling to do, if proof came before him, that he had been induced into error, he was, in a manner unequivocally hearty, most forward to apologize. In fact, he was chivalrously honourable in all his feelings and transactions, and he never hesitated to admit that he had been in error. The art of engineering men, such as the superior workmen at the Thames Tunnel, he possessed in an eminent degree, and had he been endowed with the same faculty, when he was brought into contact with men in general and with directors of public companies in particular, he would have attained success in life, in proportion to his capacity in engineering matters.

Of all the gifts presented to Mr. Gravatt, a pair of calipers made by Troughton's own hands, Bryan Donkin's chronometer by Hardy, and a specimen of Babbage's original difference engine, were those which he regarded with the sincerest pleasure. These tokens consoled him when, finding himself passed in the race of life by men who did not possess his qualifications, he was led to the inference that they did so, not because they were sounder mechanics than he, but because they were more men of the world.

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[1] *Vide* "The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science," vol. xvi.; Fourth Series, p. 34—"On the Atlantic Cable."

It would be very difficult to discuss, in a form suitable for the succinct Memoirs of the Institution, even a portion of the works upon which Mr. Gravatt was engaged. They were remarkable, not for their number, or for their magnitude, in the ordinary sense of that term, but for the amount of refined professional skill; and, if the word may be used, for the "affectionate" scientific elaboration which they embodied. Some of the boldest and best contrived of his works, the great telescope, and the bridge over the River Parret, near Bridgewater, for instance, have not, in fact, survived.

The best memorial of his broad and deep professional knowledge, his varied acquirements, his versatile and kindly humour, would be a record of his table-talk, especially of such portions of it as belonged to the occasions when, in the company of various congenial friends, who, he felt, could appreciate and follow the current of his ideas, he gave full play to his mind. But of this the only record which exists is that which survives in the recollections of his friends. And such recollections, however intimate the friendship and congenial the temper of those who are their depositaries, are at best but ineffective substitutes for the portraiture which short-hand notes alone could adequately supply.

It would perhaps seem disproportionate, and perhaps unwarranted to say, that but for Plato the wisdom of Socrates would have been lost. Certainly, but for the indefatigable and almost unexampled labours of Boswell, we should scarcely have half known Johnson. And there undoubtedly have been men of great capacity and varied acquirements, who have possessed, besides great power of communicating their ideas in familiar conversation, whose gifts and whose accomplishments, though they have exerted a wide and deep influence on the current of contemporary thought, have yet left few permanent marks associated with their name and with their history,—" carent quia vate sacro."

Mr. Gravatt undoubtedly was one of these; and though many Engineers of the present day recollect, with more or less distinctness, how largely their stores of professional knowledge have been stocked from his warehouse; how greatly their forms of thought and methods of investigation have been influenced by his views, they could but poorly convey to others an inheritance which they feel to have been of life-long value to themselves.

The career of Mr. Gravatt was brought to an untimely end at

Westminster, on the 30th May, 1866, under very painful circumstances. He was "poisoned by an overdose of morphia given inadvertently by his nurse,"—a faithful and attached servant, to whom, in the jury's opinion, "sufficient caution was not given by the medical man in attendance."

There was in Mr. Gravatt all the material to make an engineer; he possessed great acquirements, succeeded in almost everything that he undertook, was full of high, chivalrous and honourable feelings, he had a kind heart and generous mind; and he ought to have taken a very high position in his profession; but in this he failed because he could not, or would not, conform to the ordinary ways of the men among whom his lot was cast.

MR. ALAN STEVENSON, LL.B., was born at Edinburgh, in 1807, and was the eldest son of the late Mr. Robert Stevenson. (M. Inst. C.E.) He was educated at the High School and University of Edinburgh, where he took the degree of Master of Arts, and, as an advanced student of natural philosophy, under the late Sir John Leslie, obtained the 'Fellowe's Prize.' He prosecuted his studies at Twickenham, under the superintendence of a clergyman of the Church of England, and afterwards entered his father's office, to study for the profession of a Civil Engineer. In the course of his pupilage he had opportunities of seeing a great variety and extent of engineering works, comprising lighthouses, harbours, bridges, rivers, and canals; and in order still further to increase his practice, he, by the kindness of Mr. Telford, was sent on to the works of the Birmingham Canal, where he obtained much practical experience under the late Mr. William MacKenzie, at that time the Resident Engineer upon the works, and who was subsequently an extensive contractor.

In partnership with his father, Mr. Robert Stevenson, and his brother, Mr. David Stevenson (M. Inst. C.E.), he was actively engaged in general engineering business until 1843, when, on the retirement of Mr. Robert Stevenson from the office of Engineer to the Commissioners of Northern Lighthouses, he was appointed his successor; and after that period his practice was entirely confined to lighthouse engineering. Under his advice and management many important improvements were made in the lighthouse apparatus used in Scotland, especially that on the dioptric system; and the first dioptric light introduced by the Trinity House of London, at the Start Point Lighthouse, in Devonshire, in 1836, was executed from his design, and under his superintendence.

He designed and constructed many lighthouses in Scotland, but his chief lighthouse work was the Skerryvore, which was executed from his own designs and under his own eye. In personally conducting that great work, during a period of five working seasons, his courage and patience were severely tried, and his abilities as an Engineer were fully tested. They were found equal to the task of successfully accomplishing what will ever be regarded as a triumph of lighthouse engineering, and as perhaps the finest combination of mass with elegance to be met with in architectural or engineering structures. After trying four different curves, the parabolic, the logarithmic, the hyperbolic, and the conchoidal, Mr. Alan Stevenson adopted the hyperbolic curve for the tower, which has a diameter of 42 feet at the base, decreasing gradually to 16 feet at the belt course, the whole height from the foundation to the top of the dome being 155 feet. Alluding to the Bell Rock and Skerryvore Lighthouses, a writer in the "Quarterly Review" says: "Taken altogether, they are, perhaps, the most perfect specimens of modern architecture which exist. Tall and graceful as the minaret of an Eastern mosque, they possess far more solidity and beauty of construction; and, in addition to this, their form is as appropriate to the purposes for which it was designed as anything ever done by the Greeks, and consequently meets the requirements of good architecture quite as much as a column of the Parthenon." In proof of the correctness of this criticism, it may suffice to say that the proportions of the Skerryvore tower were adopted by Captain Fraser, R.E., for the Alguada Reef Lighthouse, lately constructed by him for the Indian Government, as stated in his report of the 31st of October, 1857.

The Emperor of Russia and the Kings of Prussia and of Holland presented Mr. Stevenson with medals in acknowledgment of his merit as a Lighthouse Engineer, and the University of Glasgow conferred on him the degree of Bachelor of Laws.

His principal contributions to engineering literature were his "Account of the Skerryvore Lighthouse," and the "Treatise on Lighthouse Illumination," published in 1848, and republished by Mr. Weale, in his Rudimentary Treatises. He was also a contributor to the "Encyclopædia Britannica," the "Edinburgh Philosophical Journal," and other scientific and literary periodicals. Mr. Alan Stevenson, at an early period, evinced a decided feeling for literary and classical studies—a taste which he retained throughout his whole life—and often did he relieve the monotony of professional duties by pursuing his favourite studies. A volume of original poems, and translations from the Greek and Latin poets, printed shortly before his death, for private circulation among his friends, contained many pleasing odes from his pen, all of which breathe the truly earnest and Christian spirit which characterized his daily walk.

Mr. Stevenson was seized with paralysis in 1852 at the comparatively early age of forty-five years. He resigned the post of Engineer to the Commissioners of Northern Lighthouses in the year following; and, after a painful illness, he died on the 23rd of December, 1865, in his fifty-ninth year. The Commissioners of Northern Lighthouses, whom he had zealously served, recorded in their Minutes on the 3rd of January, 1866, "their deep and abiding regrets for the loss of a man whose services had been to them invaluable; whose works combining profound science with practical skill have not only conferred lasting honour and benefit on his country, but contributed largely towards the welfare of all, and whose genuine piety, kind heart, and high intellect made him beloved and respected by all his friends, and obtained for him the willing homage of all to whom his reputation was known."

Mr. Alan Stevenson was elected a Fellow of the Royal Society of Edinburgh in 1838, and acted as a Member of Council from 1843 to 1845; and after his illness, when he tendered his resignation, the Council, in token of their respect, declined to accept his resignation, and continued to him the privileges of his Fellowship. He was elected a Member of the Institution of Civil Engineers in the year 1830, but was not often able to attend the meetings, or to take part in the proceedings, in consequence of his constant residence in Scotland.

MR. WILLIAM FISHER HOBBS was born at White Colne, in Essex in the year 1809; he was the son of a Kentish yeoman, by whom he was early initiated into the business of the farm; but his education was completed in Leicestershire under Mr. Stone, the celebrated sheep-breeder. After studying for some time further in Suffolk, his friends, considering him qualified, at the age of twenty-two years, to conduct a business for himself, took for him the farm of Marks Hall, forming part of the estate of the late Lord Western, in the parish of Coggeshall, in Essex, containing nearly 500 acres, chiefly grazing land, which afforded ample scope for the exercise of the knowledge he had acquired. At the first meeting of the Royal Agricultural Society, at Oxford, Mr. Hobbs took the first prize for cereals, and after that time the grain raised on his farm was always in request, as well for seed with the farmers as with the miller and the merchant. His success at Oxford encouraged him to persevere in the improvement of all kinds of grain and roots cultivated in the district; for which purpose he offered a bonus of one shilling to his harvestmen for every ear of wheat containing at least one hundred grains. On one occasion a workman brought him six ears containing an average of one hundred and seven grains each. These he sowed separately, and found that not only were the best ears the most prolific and produced the largest ears, but that the largest grains and the best formed also yielded the strongest and best products.

In 1844 his uncle, Mr. Fisher, died, leaving him a large property. The new position of Mr. Hobbs made no difference in the predilections and pursuits of his life. In becoming a rich landowner, he still remained a farmer; and the estate of Boxted Lodge is still one of the best cultivated in the county. It was in the transformation of this property that Mr. Hobbs displayed his science as a practical farmer. Up to the period of his taking it in hand, it had been neglected and unproductive. The light land wanted consistence; the heavy lands were soddened with water; the bottoms of the ravines were turf-bogs. The estate was divided into innumerable inclosures by high hedges, with a ditch on each side to carry off the water; whilst thickets occupied a large space that should have been devoted to the plough. All the farm-buildings were in ruins; and disorder and sterility prevailed throughout. Soon everything was changed. The peat-bogs were converted into fertile meadows, the old hedges disappeared, the ditches were filled up and the land drained, the springs opened and utilized, the sandy soils solidified, excellent farm-buildings erected where the old ones had stood, the small inclosures were converted into large rectangular fields, the mansion, the gardens, the homestead—all were transformed.

Mr. Hobbs will deservedly occupy a prominent place in the annals of modern agriculture. There were few so well grounded, and none perhaps who united so thoroughly practical a knowledge of the business of the farm with those scientific acquirements which he did so much to develop. He early placed himself in the hands of agricultural chemists, and successfully used the manures, guano and superphosphate of lime, when first introduced. He was one of the first and most earnest supporters of the now universally-accepted theory of deep systematic drainage, and was one of the best judges of stock that ever entered a show-yard, being equally at home with cattle, sheep, pigs, or cart-horses. He was well up in implements, while he could set a furrow and put a labourer right in almost any work upon which the man might be employed. But, famous as he was for his improved Essex pigs, it was in the more general service of agriculture that Mr. Fisher Hobbs came to be distinguished. He was one of the first real farmers invited to join the Committee of the Royal Agricultural Society of England, and for more than twenty years was one of its most useful members. As a member of the Council, as the chairman of a committee, or as a steward of the show, his energy was indomitable, and his administrative ability as excellent. He was equally active at the Smithfield Club, where he was in turn an exhibitor, a steward, and a judge: while he was one of the founders and most active members of the London Farmers' Club, at the time when, from its independent views, it was called the

"Tenant Farmers' Bridge Street Parliament." At the time of his decease Mr. Fisher Hobbs was a Vice-President of the Royal Agricultural Society of England, a member of the Council of the Smithfield Club, one of the Committee of the Farmers' Club, one of the Council of the Royal Agricultural Benevolent Institution, as well as a supporter of many similar societies in his own and other counties. In 1855 Mr. Hobbs was nominated a delegate from the Royal Agricultural Society to the Universal Exhibition at Paris. The following year he formed part of the jury at the Great Exhibition of the Palace of Industry; and in 1857 of that of the famous International Show at Poissy. Like most men, Mr Fisher Hobbs had his failings; he had not the art of making or retaining friends. He did not understand the uses and the pleasures of that generous hospitality which makes other centres of agricultural improvement so world-famous. A few years in the House of Commons, where on many questions his practical knowledge would have been most useful, would have corrected the defects bred by the pride of wealth and the adulation of humble and obsequious followers. But as a friend—it might almost be said as a slave—to the cause of agriculture, no one ever laboured more conscientiously or to a better purpose. His great practical knowledge, his untiring industry, and a certain quickness of observation, were of immense service, backed as these qualifications were by ample means, and with no family ties to interfere with his public pursuits, for he died unmarried. The following is a trait of his quick discernment. At the time of the repeal of the Corn Laws, all his neighbours were struck with stupor under the apprehension of certain ruin. They dismissed their house-servants and labourers, who, having no employment, went in crowds to the union-houses, to the great increase of the parish burthens. Mr. Fisher Hobbs, on the contrary, assembled those unfortunate labourers destitute of bread or work, and set them to work to clear and cut down a large wood on his estate. His neighbours thought him mad, for they were all reducing, rather than increasing, the area of their arable land. Scarcely had he finished his undertaking, when they recovered from their panic, and again took on their labourers. The field cleared by these unfortunate men is called to this day "The Free-trade Close," and is most productive.

Mr. Hobbs was elected an Associate of the Institution of Civil Engineers on the 4th of March, 1851. He died at his house, Boxted Lodge, near Colchester, on Thursday, the 11th of October, 1866, in the fifty-eighth year of his age. He had been in delicate health for some time from the rupture of a blood-vessel, and for the last few months of his life had been almost entirely confined to his room, and for a year or two previously he had been seen but little in public.

Mr. ARTHUR JAMES, eldest son of Mr. John Thomas James, Purser of H.M.S. 'L'Espoir,' was born at St. David's, Pembrokeshire, on the 3rd of March, 1816. At the age of two years he was adopted by his father's brother-in-law, Nathaniel Philipps Bland, Esq., of Trelethin, who, being a Gradùate of Oxford, and a man of varied attainments, carefully superintended his early education. When he was twelve years old he was placed at the Collegiate and Chapter School of St. David's, under the charge of his maternal uncle, the Rev. Prebendary Richardson, and afterwards at the grammar school of Haverfordwest, then under the charge of the Rev. James Thomas, M.A. Subsequently to this he appears to have spent several years at Trelethin, engaged in private study, showing a strong taste for mathematics, and for scientific pursuits in general, as well as some skill in mechanics. On one occasion it is related of him, that, having constructed an enormously large kite, he applied it successfully to the traction of a carriage across the neighbouring downs, and also to the conveyance of himself across St. David's Bay, through the restless waves of which (after having attached his clothes to the line of the kite), he permitted himself to be towed, aided by a brisk breeze, from the shore under Trelethin towards St. David's Head, near which he safely effected a landing. He early showed a most fearless nature, and at that time is said to have excelled both as a swimmer and a cragsman, filling up the intervals of his studies by boating, fishing, and seal-hunting.

In 1844 he was engaged by Mr. Harry Phelps Goode, of Haverfordwest, on the Parliamentary Surveys of the Manchester and Milford Railway, and on this, though only working out his own theoretical acquirements as connected with mathematical study, he produced excellent field-work, and passed muster with the surveying party for an "old hand."

In 1846, Mr. James was introduced by Mr. Goode to Mr. Robert Brodie (M. Inst. C.E.), who had the engineering charge, under the late Mr. Brunel (V.P. Inst. C.E.), of the Swansea division of the South Wales Railway, then in course of construction. Appreciating his mathematical acquirements, his energy of character, and his aptness for acquiring engineering knowledge, Mr. Brodie employed him on the works of that division, where he was chiefly engaged in setting out and correctly maintaining the centre line and levels, throughout the execution of the works, until 1849. He was then transferred to the Newport division of the same railway, under the charge of Mr. W. G. Owen (M. Inst. C.E.), by whom he was intrusted with the winding-up of all the measurements of contracts for works from Gloucester to Neath. These had been executed by twelve separate contractors, and amounted in value to about £750,000. Mr. Owen states that "it was very much owing to Mr. James's great patience and skill, and to his nice tact and good temper in dealing with the agents of the contractors, that we were enabled to close this troublesome business satisfactorily, and without a single law-suit."

In 1851, on the commencement of the works of the Forest of Dean Railway, Mr. James was engaged as Resident Engineer. These works were peculiarly troublesome and difficult, embracing the enlargement of three long tunnels, which were originally merely tramway headings, and the conversion of nine miles of tramway, into a broad-gauge line, the gradients and curves varying considerably, and the traffic being kept open throughout. Mr. James devoted himself day and night to this operation, and carried it through very successfully.

After the completion of these works, in 1854, Mr. Brunel having strongly recommended him to Government for an appointment connected with civil engineering in the Crimea, during the campaign, Mr. James was about to enter into such an engagement, when he received a permanent appointment as Engineering Assistant, at Paddington, under Mr. T. H. Bertram (M. Inst. C.E.), of the Great Western Railway. In this position, the completion of the new terminal works, the extended application of the hydraulic system of Sir William Armstrong to the passenger and goods stations, and to the coal depôt, the rearrangement of many stations, and laying out of new branch lines, with all the multifarious routine and office duties, afforded ample and varied scope for his energetic and efficient services: and he proved himself a most valuable assistant in every way; ready and skilful in mathematical investigation, indefatigable in carrying out all that was intrusted to him, and withal most obliging, genial, and popular. From 1860, when Mr. Michael Lane (M. Inst. C.E.), was appointed Principal Engineer, on the retirement of Mr. Bertram, Mr. James continued in the discharge of similar duties, having also to superintend the reconstruction in brickwork of a long timber viaduct at Windsor, while kept open for the running of frequent trains, and the execution of the works for the junctions of the Metropolitan, the Hammersmith, and the West London Railways with the Great Western line. At the same time he afforded efficient aid in directing surveys, and in preparing the details of Parliamentary work for promoting the new Bills of the Company, and for opposing hostile schemes.

On the 4th of January, 1866, he was appointed, as successor to Mr. Alexander Mackintosh (M. Inst. C.E.), General Assistant to Mr. Lane, with the superintendence of the Way and Works of the Railway. Shortly after, on the 20th of March, while in discharge of duties on the line in connection with that appointment, and engrossed in the examination of some details of permanent way, near Ealing Station, Mr. James was accidentally run down by an express train, and was instantly killed.

Mr. James was elected an Associate of the Institution of Civil Engineers on the 13th of April, 1858, and at the time of his death was in his fifty-first year. His remains were interred in Ealing Cemetery, in the presence of the chief officials, and of upwards of five hundred of the employés of the Great Western Railway Company. By the urbanity of his manner and the kindness of his heart, Mr. James had endeared himself to every one with whom he was associated. A valued colleague, a warm friend, and a generous and considerate man, the tidings of his lamentable death were received at the Paddington Station, where he was so well known and appreciated, with the utmost consternation; while to the Company there was thus lost the services of an active, faithful officer, ever anxious to do justice to the employer as well as to the employed.

Mr. JOHN FRANCIS PORTER was born at Carlisle on the 11th of August, 1810. His father was an iron-master, who effected many improvements in the production of bars, chain-cables, &c. Through his enterprise the Carlisle and Solway Ship Canal was originated; and subsequently, the Newcastle and Carlisle Railway, for which he was selected by the Earl of Carlisle as his Lordship's director. He also, in co-operation with the Liverpool authorities, succeeded, against much opposition, in establishing the public testing-machine at Birkenhead. Mr. J. F. Porter did not undergo the ordinary apprenticeship either to a civil engineer, but having the run of his father's works, and constantly aiming at self-improvement, he probably acquired a more general knowledge of engineering than he would have done in any other way. At the age of sixteen, while still at school, he showed in a remarkable manner his application in acquiring a thorough knowledge of mechanics; but until a later period of his life than that just named, he was afflicted with a stammering of speech, so much so that at times he could not articulate at all. After some months' consultation in London, and lengthened residence with Mr. A. M. Hartley, the Glasgow elocutionist, this stammering was partially overcome, but a hesitation of speech affected him throughout life, and it was at times very distressing to himself, as well as embarrassing to strangers with whom he had professional intercourse.

In 1839 he removed from Carlisle to London, and entered into an engagement with the late Mr. T. Cubitt (Assoc. Inst. C.E.), as principal Engineer of his works at Thames Bank, and at Burham, Kent. He remained connected with Mr. Cubitt for nearly twenty years, during which period he planned and superintended the execution of the whole of the machinery for those extensive works, and therein effected many improvements. In a mercantile point of view he did much service in developing and perfecting the brick-making machine, originally known as Ainslie's; so that the Burham resources had no difficulty in meeting the enormous requirement for bricks at Belgravia, both as regarded the large quantity produced and their superior quality. On relinquishing this engagement, in the year 1853, he commenced business on his own account in Westminster, as a civil and consulting engineer, holding the office of consulting engineer to the late Mr. Alderman William Cubitt, M.P. (Assoc. Inst. C.E.). He next became connected with the erection of some public baths and wash-houses for the French Government, which ultimately claimed the whole of his attention. At this time he removed with his family to Paris, where he remained for a considerable period. On his return from France, at the close of the year 1857, he was for some months engaged with his brother, Mr. George Porter, of Carlisle, in constructing brick-making machines for the East Indian Railway Company; but he soon entered into an engagement with the South Staffordshire Waterworks Company, as Resident Engineer for the extension of their supplies; and ultimately, under Mr. J. R. M'Clean (Past-President Inst. C.E.), he was employed in the formation and superintendence of the Burton-on-Trent extension of these works. Having nearly brought them to a conclusion, he was taken suddenly ill, and, after much suffering, died at his residence in Walsall, on January 26th, 1865. His remains were borne to the family vault in the cemetery at Wallason, Cheshire.

Mr. Porter was elected an Associate of the Institution of Civil Engineers on the 4th of April, 1843, and he frequently attended the meetings and took part in the proceedings of the Institution.

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